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Curt Stern

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Vol. 3, No. 3, September, 1956 *Eugenics Quarterly*

GENETICS IN THE ATOMIC AGE*

This is the first article in a series on Genetics and Radiation.

CURT STERN
The University of California

GENETICS HAD AN INNOCENT YOUTH, as many a science. What quieter birthplace could be imagined than the walled garden of the monastery in which Johann Gregor Mendel grew the peas from which he learned the first laws of inheritance? Yet the seclusion of the origin of genetics was deceptive. When, in 1865, Mendel wrote down his modest report "Experiments on Plant Hybrids," he knew that Darwin's theory of evolution had caused an outbreak of emotions among millions of thinking people. He realized that the findings on crosses of garden peas had their full bearing on the issues of creation and evolution which inflamed the minds of men.

It has often been regretted that Mendel's work remained largely unnoticed until it was rediscovered in the first year of the new century. There is at least one compensation for this delay. It saved genetics from being drawn into the Darwinian controversy when it was at its most embittered stage. By 1900 the storm had diminished greatly and genetics could develop healthily within the confines of the universities and research institutes.

When, however, several decades later, genetics came of age, it could not escape the raids of illegitimate invaders. First, its findings on animals and plants were uncritically and malevolently applied to man, and racial bigotry and crime were justified by appeal to a pure science. Then, the opposite happened. Lysenko, in Russia, for reasons which have never been fully understood, was empowered to crush the brilliant group of geneticists in his country and to label them as reactionary and despicable.

Genetics thus may serve as a striking example of the close interrelations of all human activities. Science and politics, philosophy and religion, art and social doctrines, technology and the theories and practices of government do not exist in separate compartments but interact constantly in many ways, sometimes subtly, at other times violently, to mankind's benefit one day and to its detriment another.

We are just now at the beginning of an era in which genetics once more comes into the foreground of public attention. This time the demands on this science are fully legitimate. We have known for nearly 30 years that radiation, from X-ray tubes and from radioactive substances, can change the hereditary elements of animals and plants. Herman J. Muller, the discoverer of this artificial transmutation of the genes, was rightly hailed as the first man to succeed in interfering at will with those processes of nature which until then had restricted genic changes to rare and inaccessible events. Since that time the science of radiation genetics has expanded into a whole field of its own. Much of its results lie in the realm of fundamental knowledge. The students of evolution in animals and plants, of the microscopic anatomy of the cell nucleus, of the action of genes in the functioning of cells and in development—all have gained new tools and new insights into their problems. But Muller himself was and is not only a great searcher of the unknown. He is at the same time a man aware of immediate social obligations. In the

*An address given on April 14, 1955 before the International Council for Exceptional Children.

first announcement on the induction of mutations by irradiation of the fly Drosophila he called attention to the significance of this finding to all men and women. What radiation does to the genes of flies, it undoubtedly also does to the genes of man. X-rays are used for diagnosis and treatment, including sometimes for producing fertility by stimulating ovarian function and for causing temporary sterility by suppressing the formation of sperm. We must count on mutations in the germ cells being produced by these irradiations, mutations which will appear in the descendants of the irradiated individuals. Muller warned medical men of the consequences of their acts. You must weigh, he said, the benefits to the treated individual against the harm to the race. The fight was joined. Why should there be harm in mutations, the opponents asked. Does evolution not mean progress and does it not depend on mutations? Moreover, some physicians added, give us proof that irradiation causes mutations not only in flies but also in man. The gynecologists would say: we have treated with X-rays the ovaries of numerous women and to their happiness many children were born to them. All were fine, healthy specimens. Some of these by now are adult women themselves and have had babies of their own. Again, no ill effects, no noticeable changes have appeared.

Harm by mutations, the geneticists replied, is a matter both of fact and theory. Actual study has shown that the overwhelming majority of changed genes are detrimental to the well-being of the individual. Some kill outright, early or late in embryonic development. Others cause visible abnormalities of external or internal organs. Still more lead to defective functioning of blood or nerves or stomach or glands. This evidence is clear. It is supported by reason. Man, like any other organism, is the achievement of long periods of evolution. He has been brought to a peak of internal adaptive perfection. Change any part of it and the result most likely will be for the worse. Who has ever heard of a finely adjusted watch being improved or even left unimpaired when one of its wheels is replaced by another of different kind? Should one expect better results from changes in the genes of an organism? Yet, the appeal to the past power of evolution raises a new question. The adaptive peak which any species represents is never absolute. Evolution is the attainment of other new peaks. It is a continuous process. It depends on genic changes as its raw material, and it implies the existence of harmless mutations. These statements are true, but evolution takes on different values if applied to plants and animals or to man. Evolution is a cruel process, sacrificing innumerable unsuccessful trial specimens for the survival of the few successful ones. We can afford to irradiate bacteria and moulds in order to create mutations for the production of desired chemicals or for higher yields of antibiotics. Billions of useless individuals can be discarded unconcernedly so as to select a single desirable one. Likewise we can induce mutations in barley or peanut plants and without remorse save only the very small minority which suits our purposes. But applied to man, the harmful induced mutations must weigh on our conscience. The possibility of some favorable ones cannot be balanced against the certainty of the majority of unfavorable mutations.

Why, however, have the physicians not seen evidence for mutations caused by irradiation of human individuals? The geneticist's answer to this question needs some elaboration. As a preamble he would voice the conviction that man as a biological species has never been found to be outside of the laws of organisms. Mutations can be produced by radiation in viruses, bacteria, yeasts, tobacco plants, flies, wasps and mice. It seems impossible that the genes of man should be immune to changes which the genes of all other forms of life must endure. The fact that no mutations have been the obvious consequences of ir-

radiation by X-rays in man must have a particular reason. It is indeed at heart a very simple one: insufficiency of numbers. Well-documented reports on the "many" children produced by mothers or fathers whose sex glands had been relatively heavily treated with X-rays probably cover no more than a few hundred babies, and the number of recorded grandchildren of the treated parents is perhaps less than a score. This is not the kind of material to which the geneticist is accustomed who rather deals with thousands and millions of individuals. Moreover, his plants and fruit-flies can be exposed to much heavier doses of X-rays than are ever applied to his fellow men, and, therefore, he expects less numerous genic changes in humans.

The offspring of treated patients are not the only ones in which one can search for mutations. There are also the children of professional radiologists who themselves over the course of years often had been exposed to considerable radiation from X-ray tubes. There are some surveys of such children, but again the records are limited and the results not clear. Finally, there is one more group of people, the largest and most intensely studied: the children of those who, living in Hiroshima and Nagasaki in the fatal days at the close of the great war, survived the inferno of the two atomic bombs. The radiations sent out by these weapons are similar in effect to those of X-rays and should have resulted in human mutations. When the war was over, the National Academy of Sciences as directed by the President of the United States, and with the cooperation of the Japanese Government, Japanese physicians and midwives, established a research organization to determine the after-effects of the atomic bombings. Willingly and yet hesitatingly, the geneticists participated in this effort. They were willing to rescue knowledge from the misfortune of war but hesitating since it was uncertain how much knowledge could be gained. It was possible to make calculations in advance to gauge the results of mutations in the descendants of the exposed parents. Such calculations had to be based on the best studies available, data from flies, not man. Some adjustments could be made in these calculations, taking into account aspects in which the genes of flies and man differed. At best, reasonable guesses, not sharp predictions could come from such procedures.

What, then, were the expectations? Let us assume that the sex glands of a group of the heaviest irradiated survivors in the two cities had received an average dose of 100 so-called röntgen units of radiation, a dose comparable to that accumulated by the sex glands of patients who have been given a good many abdominal X-ray exposures. It may be estimated that perhaps 15 per cent of all children from an exposed and a non-exposed parent, or perhaps 30 per cent from two exposed parents, carry some one induced, mutated gene. Few mutated genes, however, produce their effects in the children of the exposed parents. Three main kinds of genes can be distinguished: fully dominant, irregularly dominant and recessive genes. All fully dominant genes but only a small fraction of the other two kinds become noticeable in the first generation. The fully dominant ones probably represent less than one in a hundred of all mutated genes. In detail, the action of these genes varies. There are those which severely disturb the development of the embryo and thus lead to death and abortion, sometimes so very early that the mother may not even have been aware that she had conceived. There are others which will bring forth malformations or malfunctioning, slight or serious. Very rarely only will the word "monster" be in order.

One in a hundred fully dominant out of 15 or 30 per cent of induced genes corresponds to only about one in 700 or one in 350 children visibly different from what they would have been, had their parents never been exposed to artificial irradiation. To these may be added from one exposed parent perhaps another one in 900 children

or from two exposed parents one in 450 who might be malformed because an irregularly dominant gene gained expression or because an induced recessive gene had met in fertilization another recessive of its kind. Altogether the induced dominant and those few recessive genes which might be expected to produce visible effects in the first generation would account for around 2 to 5 malformations among a thousand children. Now, it is a fact well known to students of public health that more than 1 per cent of children from average non-exposed parents are visibly, though often very slightly, malformed at birth and that very many more show some unfavorable qualities in later life. If we could observe 1,000 children from parents both of whom were exposed to the 100 "r" dose we would, in any case, expect more than 10 of them to be malformed at birth independently of the radiation exposure of the parents and perhaps an additional 5 children to be abnormal due to the heavy radiation exposure of both progenitors. The difference between 10 or 15 abnormal out of 1000 children born would not entitle one to any definite conclusions. Chance, and other causes, constantly bring about small fluctuations in the number of defectives born so that even without any radiation exposure the number of abnormal children among 1000 births may easily from one year to the next be as low as 5 or as high as 15. Thus, as long as the total number of children born to heavily exposed parents is relatively small the calculated increase of abnormals would remain undetectable.

This is the situation concerning the children born in Hiroshima and Nagasaki. Although tens of thousands of children exclusive of those who were on their way in 1945 were later born to people who were exposed to the bombings in the two cities, only about 600 children came from parents both of whom had survived heavy irradiation. Among these 600 children were 5 abnormal ones, or somewhat less than 1 per cent. Obviously, the finding of only five instead of the average expectation from non-irradiated parents of 6 malformed children does not suggest that radiation reduced the number of abnormals. It is simply a chance fluctuation compatible with what could have been predicted. One obtains somewhat larger numbers if children are included who had only one instead of two heavily exposed parents. This raises the total to nearly 1800 children, 21 of whom showed some malformations. Clearly, again this number constitutes no evidence for an effect of the exposure of the parents upon their children since the difference between the average expectation of 18 malformed children and the actually found number of 21 may very likely have been due to chance. Even the more extensive data are still too small to permit the drawing of a valid conclusion.

The studies in these Japanese cities are still under way. There are many more children whose parents had been less heavily exposed than those considered here. No complete report has yet been published on them. However, the statistical advantage of the larger number of children in this group is counter-balanced by the statistical disadvantage that the theoretically calculated effect of the weaker irradiation would be much less than among the children we have discussed. So it may be expected again that no obvious evidence for an increased number of malformations will be found.

Can we then draw no conclusions from the painful, laborious and costly studies in these cities? There are indeed important conclusions. One of them is as follows: Man's genetic sensitivity to irradiation cannot be very much larger than that of other organisms. If it were a hundred times higher than in flies, then even the limited Japanese data would have shown a clear increase of defective offspring. Mice, it appears from recent studies, are genetically more sensitive than flies, but again it seems as if the sensitivity of human genes cannot be very much larger than that of mice. Another conclusion is this: If man's sensitivity

is about the same as that of other organisms, then the findings in Japan are fully within genetic expectation, or, stated a different way, there is no reason to assume that unfavorable mutations have not been induced. Had more heavily exposed couples survived in Japan so that they might have produced not 600 but 600,000 children, then there is good reason to believe that beyond the 6000 malformed children expected in any population of this size additional many hundreds would have been malformed as the result of the radiation damage to their parents. With such large numbers we might well have expected undeniable evidence for the ill-effects of radiation on human genes.

We have dealt with the immediate offspring of the unfortunate inhabitants of the two Japanese cities. What may be expected of future generations? It is here that we encounter particularly the problem not only of fully dominant but also of the irregular dominant and of the recessive mutations presumably induced. The fully dominant mutations, those which should have appeared immediately, would have a chance to reappear in about half of the children of those who showed their effect themselves. Since the number of such first generation individuals with induced defects was so small as to have been beyond recognition, the effect of fully dominant mutations on future generations is equally negligible. The facts are peculiarly different for the irregular dominant and the recessive mutations. As was said earlier, there may well have been induced over one hundred times more such mutations than fully dominant ones. Therefore, among the 600 children from two heavily exposed parents there may have been no less than 200 or 300 who unknown to themselves and others carry an induced irregularly dominant or recessive gene. These children, when adults, will transmit their unfavorable gene to about half of their offspring, and, usually, this fact will again remain without obvious consequences. Only rarely will an irregularly dominant gene express itself in the next generation or will the spouse of a person who carries a recessive gene also contain, again unknown to himself, one such recessive gene of the same kind. This would be due to the fact that any one mutation induced by radiation has also occurred many times before spontaneously in earlier generations and has been transmitted from generation to generation. When the two spouses happen to carry the same recessive gene, then there will be a chance for the production of an abnormal child due to the coming together of two like genes from both parents. Among the possibly 200 first generation people who carry an induced irregularly dominant or a recessive gene, hardly any more than 3 or 4 will find a spouse with a recessive gene like their own. Altogether, the marriages of persons carrying an induced irregularly dominant or recessive gene may contribute 1 or 2 newly defective individuals. Since, however, among the future children from the 600 first generation individuals from heavily exposed Japanese there will be "spontaneously," regardless of the exposure, around one per cent abnormal ones, the additional 1 or 2 induced defectives will not be discernible. In a similar manner, each successive generation is likely to bring forth the appearance of a few more defectives but their origin as due to atomic radiation will be even less recognizable. By then the remote descendants of the exposed individuals will have become so interspersed with the rest of the Japanese population that the few induced defectives will be included as an unidentifiable small fraction among the defectives of the whole country.

This, however, is not the end. While some of the actually born induced abnormals may be so badly handicapped that they die early or leave no children, other genes may possess less fatal effects although still undesirable ones. Lack of offspring among the former means that the induced mutation will die with them. With fertility of the latter their unfavorable gene may be

transmitted to further generations. Very likely, if it was an irregularly dominant gene it would disappear again from view and if a recessive it would first meet a normal gene. In either case a normal-appearing child would be born. The child, however, would transmit his abnormal gene as well as his normal gene, and, in the course of generations, again a defective child with the long ago induced bad gene would appear. So, for hundreds and thousands of years there would be occasional reappearances of the mutated condition.

There are two aspects to this situation. The first concerns the statistical phenomenon. Among the billions of individuals born in each generation on our earth, the few induced abnormal ones from the events in August 1945 have not and will not be discernible. Even in Japan alone no evidence for mutant genes is likely to be discoverable. The other aspect is the peculiarly human one which does not deal with man as a statistic but with each individual as a unique person endowed with inalienable rights to life, liberty, and the pursuit of happiness. If we approach our topic, "Genetics in the Atomic Age," from this human and also, historically, truly American point of view then we can only stand bowed in sorrow and add these future unfortunates to those endless others which human folly and ill will has killed and maimed through the centuries.

The two atomic bombings which have furnished much of the basis of this discussion are events of the past. We are all concerned now with the effects which the yearly tests of nuclear weapons might have on the genetic endowment of man. All of us are normally exposed to natural radiation, from cosmic rays, from the radioactive substances which, in small amounts, occur in our surroundings, in soil, air, in the material out of which our houses are built, and from those radioactive atoms which our own bodies accumulate from food and water and air. What is the effect of this unavoidable background irradiation on our genes? It is generally believed by geneticists that it is responsible for some of the so-called spontaneous mutations which occur perpetually in us. Not all spontaneous mutations are radiation induced, but the relative proportions of the radiation-dependent and the independent human mutations is not known. Why is there general belief among geneticists in the power of background radiation to cause mutations? Is this radiation not so low in intensity that it takes years of life until the total dose of irradiation has added up to the same amount which a single X-ray photograph of the abdomen may impart on an individual? Should we not assume that the immense "dilution" of exposure which the background irradiation represents will make it harmless? There are several reasons why this assumption is considered wrong. Thus, it has been demonstrated experimentally that given amounts of radiation produce the same number of mutations regardless of whether the radiation was administered in the concentration of 300 röntgen units per minute or, diluted 300,000 times, administered at one-thousandth of a röntgen unit per minute. Another reason lies in our knowledge of the process by which radiation induces mutations. This may be compared to that of the happenings by means of which a bullet kills or injures a man. If a single machine gun sprays a field with missiles, fewer combatants will be hit than when thousands of guns are employed. However, the effect of any individual hit will be independent of the concentration of the bullets. In the same way, the low-rate background irradiation will produce fewer mutations per time than a heavy dose, but there is no reason to believe that its proportional share of genetic changes will not be produced by it.

Tests of atomic weapons add to the natural background radiation. Some of the radioactive material produced in the test explosions comes down to earth or to the ocean within a circumscribed area whose size varies with the power of the device

and other circumstances. The rest of the radioactive material goes high up into the upper layers of the atmosphere and then slowly falls out over the surface of the planet. Much of the material loses its activity at a fast rate, but some will continue sending out radiation for thousands of years. What is the effect of fall-out on the hereditary material? We must assume that it will add new mutations just as does the natural background irradiation!

This statement is not made lightly. Scientists are trained to weigh their pronouncements more rigorously than other people. They find it difficult to say only yea, yea—nay, nay but hedge around with phrases such as "highly probable" or "most likely." This is often annoying to their fellowmen whose accuracy is usually sufficient to meet the tests of everyday life. But even the members of a jury which may regard the evidence as incontrovertible that a man is guilty of murder should never pronounce their damning verdict without knowing in their hearts that at best the verdict is one which is true with a probability which may border on certainty but which still leaves some room for error. The statement that the recent increases of background irradiation which can be measured all around us will cause additional mutations in our genes is one for which not every last bit of evidence is in. The opposite statement, however, that no genetic damage is produced by current weapons tests runs counter to the best knowledge available. If we use the same care and respect for evidence which juries and judges are asked to apply to their decisions on human fates, then we must also convict as guilty of falsehood magazine articles who flatly declare that no harm is done by atomic tests.

Sometimes these misstatements may be launched well-meaningly in order to counteract exaggerated claims of the damage caused by fall-out. Taken in a statistical sense the presently estimated damage is vanishingly small. Recently, Professor Sturtevant of the California Institute of Technology has calculated that, if the increase in irradiation due to fall-out continues at his estimated present rate, it will lead to the birth, each year in the United States, of some seventy children who will carry a mutation induced by fall-out atoms. Sturtevant adds that this estimate may be much too low and the actual number may be as high as 7,000. Statistically, even the last figure will have no noticeable impact. As we have seen earlier only perhaps about two per cent of these children will actually exhibit a change from normal so that, even accepting the higher estimate, observable harm may hit less than 150 individuals. If one compares this with the four million children born in our country at the present yearly rate, and the more than forty thousand of them who will be defective at birth it becomes clear that no one needs to be concerned about the effect of fall-out on the kind of children he will have or about the future of the people at large, or of mankind. Even if the types of tests which have been carried out are continued for many years and even if still more damaging amounts of radioactive material are released in tests, it will be a long time until there will be a noticeable increase of mutated individuals. Yet, as soon as we leave the statistical approach we are confronted with the fact that there will be people in the many future generations who will be born with defects and deficiencies which are the results of our present actions. What would we say if deliberately even a single innocent child, not to speak of a hundred fifty, were drawn from the newborn each year and killed, or injured for life?

There is still another side to our worries. Genes do not only exist in our germ cells, but they are equally present in each of the billions of cells which constitute our bodies. We do not transmit to our children the genes in these body cells, but we depend on their functioning for our own life and well-being. They can be changed by irradiation in the same way as the genes in germ

cells and certainly the changes again are usually for the worse. Our bodies are more complex than single cells, and the nature of the bodily, personal effects of radiation is less well understood than that of the mutations of genes which we transmit to our offspring. Whatever they are in detail, we do know of ill effects. The incidence of leukemia, in animals and in man, can greatly be raised by irradiation. Many radiologists as well as survivors of the Japanese bombings have furnished tragic testimony to this fact. Furthermore, a high mortality rate, a faster aging, as a result of irradiation, of apparently healthy animals of several species has also been observed. We are by no means certain that the effects on the frequency of leukemia and on aging can be produced by very weak doses of radiation such as coming from fall-out. But it is a possible danger to be kept in mind most seriously. Again, no one will foresee a major catastrophe. A people, however, which rightly has become concerned with the rise within recent decades of cancer of the lung must also be alert to the possibility of a rise in leukemia and of forces which might tend to shorten our lives. This awareness is necessary even though increased medical knowledge may lengthen our average life span by still more years and thus obscure a loss of weeks or months due to fall-out.

The genes have felt the import of atomic events ever since life existed. They have been under the influence of the atomic age for sixty years, since the discovery of radioactivity and of X-rays. They have been exposed to atomic warfare and to atomic tests. They will be exposed increasingly to the products of the peaceful applications of atomic energy. People will be subjected to radiations from atomic power plants, and even the best disposal of radioactive waste products from such nuclear factories will not eliminate some contamination of our planetary environment. Industrial planning and legislation will set limits to the harm which can be predicted, but mankind will have to live with it as it lives with the knowledge of injury and death afflicted daily on thousands by fire and electricity, autos and planes, drugs and chemicals, and many other discoveries and inventions of civilization.

At present the greatest amount of atomic damage is still done by the oldest and most widely spread application of atomic knowledge, namely X-rays. The good which X-rays are doing to mankind by far outweighs the harm, but probably all geneticists are convinced that we still can greatly reduce the latter. The genetic problems of the peaceful application of atomic energy are similar in scope to those posed by X-rays. The hoped-for human benefits will have to be accompanied by a price in the well-being of some individuals. This will and should be a matter for discussion and action for many years to come.

Finally, there are the genetic problems of atomic warfare. We have discussed the genetic consequences of weapons tests. There are those who, shocked by the unavoidable causation of defective human beings, speak out uncompromisingly against such tests. They do not admit the analogy with peaceful applications where benefit together with injury appears on the ledger. There are others who believe that atomic armaments have held back the advent of wars and that the balance sheet for weapons' tests does not lack its positive entries. No one, however, can escape the conviction that war which was always evil is an unimaginable evil in the atomic age; that atomic armaments constitute at best only the most temporary postponement of needed solutions; that every man and woman must constantly endeavor to free himself from international hates and prejudices; that our daily determination must be to find peaceful ways out of mankind's predicament; and that our dedication must be not to death and destruction but to life and construction.

CONTROL OF CONCEPTION BY PHYSIOLOGIC METHODS

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IT MAY BE ASSUMED that future work will provide improved methods of controlling conception by mechanical or chemical interference with sperm entry to the uterus. Some of the methods may prove to be more acceptable and cheaper, as well as more effective, than those now available. Nevertheless, few people would deny that the method or methods likely to receive the widest adoption would be ones that would be effective when taken orally and as infrequently as posible. Somewhat less acceptable, but still desirable, would be procedures which would involve the infrequent injection of substances having antifertility effects.

Ideally, one supposes, the contraceptive of the future would be a pill that taken once would bring to a halt the process of reproduction for weeks or months — or even better, until another pill is administered to awaken the slumbering mechanisms. The latter may sometime be achieved, but nothing we now know about reproductive physiology, or for that matter, any physiologic process, would lead us to believe that so easy a solution is practicable. The interposition by physiologic methods of a briefer period of sterility has brighter prospects of earlier availability, but even this is beyond the range of present knowledge.

Any contraceptive procedure to achieve general acceptability must have the property of safety, *i.e.*, it must not evoke undesirable side effects. The very fact that an oral contraceptive would be taken by mouth provides a difficulty, since once a substance is ingested it has the opportunity to affect any of the tissues of the body. There would be little difficulty in finding many substances which would have inhibiting effects on one or another of the reproductive processes, but since these are basically nothing but cells or cell mechanisms the problem resolves itself into finding substances which act specifically on the tissues of the reproductive system. Substances with a specific action limited to a given tissue of any kind are rare indeed, and in securing a desired effect dependence must be placed upon significant differences in tissue sensitivity to the substance in question. Thus it is possible to produce desirable effects on one or another organs of the body by certain drugs because experience has shown that they are effected by lesser doses than produce changes, particularly undesirable changes, in other organs. Many drugs cannot be used or are used only with great caution because the difference in threshold of effects on different organs is too narrow.

In the case of drugs which would affect the reproductive processes the requirement of safety is even greater than in most circumstances since their use would be very widespread and frequently without benefit of medical supervision. Something with the order of safety of small doses of aspirin would be essential. At the present time a number of substances are at hand which have the capacity to interfere with reproduction, but none of these can be regarded as acceptable because the danger of side effects is too great.

The search for physiologic methods of controlling fertility quite logically is focused on those points in the reproductive processes which appear to be most vulnerable. These have been recited at some length in a paper prepared for the International Planned Parenthood Conference in Tokyo. At this time, rather than offering such a detailed account, the major cate-

gories only will be given in outline form. Thus, seven phenomena, each divisible into variable numbers of processes and involving one or both sexes, are listed below. In each instance the mechanisms involved are such that suitable interference with them would prevent conception.

1. Production of germ cells (eggs, sperm)
2. Maturation of germ cells (especially sperm)
3. Transport mechanisms (eggs in oviduct; sperm in male and in female tract)
4. Hormonal mechanisms (especially in passage of egg and preparation of uterus; but also in providing suitable environment for sperm)
5. Fertilization
6. Inplantation mechanisms
7. Early embryonic development

Of these phenomena, it would seem that the most desirable point at which to interfere would be at the stage of fertilization. Thus, for example, if we can learn how to change the egg membranes so that the sperm cannot enter, the entire problem would be solved. It would be unnecessary to alter hormonal mechanisms or metabolic phenomena associated with germ cell production and transport, processes which have interrelations with other bodily mechanisms, and there would be no need to feel concern about preventing implantation of the fertilized egg or about destroying it in early developmental stages. This process, fertilization, and its associated phenomena, is receiving an increasing amount of study and deserves as much attention as can be brought to bear upon it. Thus studies now in progress are aimed at learning the nature of the egg membranes from physical, chemical and electrical standpoints and the nature of the changes which are invoked in them when fertilization occurs. Other attention is being directed toward the nature of the environment in which fertilization occurs and the factors which seem optimal for sperm-ovum union. Once these mechanisms are adequately understood, it may be possible to devise methods of altering them in such a way as to inhibit fertilization.

Much the same kinds of studies are being directed to varying extents at the other points in the outline. These are studies concerned with learning the nature of fundamental mechanisms and the conditions which are essential for their operation. Armed with that information, the invention of procedures designed to upset given processes becomes a reasonable possibility.

While most of the potentially significant researches are in the exploratory stage and concerned primarily with fundamental mechanisms, there are others which are being focused more directly on the problem of fertility interference. While it is not altogether realistic to draw sharp boundaries between basic and applied research in this field, for purposes of discussion three groups of studies may be placed in the latter category. These are studies on:

1. Steroidal hormones
2. Antimetabolites
3. Plant extractives

The first of these, steroidal hormones, has received considerable publicity recently as a result of reports from the Tokyo Conference. In brief, some steroidal substances have been reported as inhibiting ovulation in women as well as in laboratory animals. Such interference with fertility is a recognizably acceptable procedure if it can be done predictably and without disrupting other physiologic mechanisms. The use of some steroidal substances to change the menstrual cycle has been successful enough to encourage us to believe that ovulation may be inhibited. Indeed, certain enthusiasts have stated that such was the case in a large percentage of approximately thirty women treated with progesterone and in all of a handful of women treated with either 19-nor-ethinyl testosterone or 17-ethinyl estraeneolone. The evidence presented by these investigators for the ovula-

tion-inhibiting effects of these steroidal substances was questioned in Tokyo, and recently the Population Council has initiated two programs of study which should provide more objective data in this regard. Thus the overt enthusiasm exhibited in Tokyo should be tempered with the realization that in actuality nothing significantly novel was presented there.

If we admit that existing, or anticipated, steroidal compounds will inhibit ovulation (and it seems likely that such may actually be proven in women), there remains very real problems in regard to their general use in the control of fertility. Dismissing at this time the question of cost which may be solved by mass production, there is the matter of the close interrelation between the functions of the gonads and the adrenal cortex, an organ of very great importance in many physiologic mechanisms. Without elaborating upon this point, it is significant to say that in recent months a report has been made that progesterone, when given in doses comparable to those used to inhibit ovulation, has caused serious disturbances in adrenal function.

This point is made not to suggest that the steroidal substances should have no place in our thinking about the physiologic control of conception but to caution that a very effective steroidal compound may be found to be a two-edged sword. It would not require many instances of adrenal failure to discourage the use of progesterone, for example, as an antifertility compound. And this instance serves to point up the fact that it is no easy matter to influence a reproductive process without also affecting other important physiologic mechanisms.

It is not fair to leave the question of fertility control by steroidal hormones without saying that there are many mechanisms involved in reproduction other than the inhibition of ovulation, which may be influenced by these substances. Indeed, each one of the seven major categories that have been listed falls within the scope of their influence. The problem is to discover how we can make suitable application of this relationship.

If we now turn to the second area of research, that of the use of anti-metabolites, it will be evident that again we encounter investigations of great promise but which carry with them the red light of caution and danger.

The term "antimetabolite" is referable to any substance which interferes with metabolic processes, but it has come, in the case of antifertlity research, to be attached to those compounds that have a profound effect on the young embryo. There are several of these with very potent activity in animals, but the comparable effectiveness in human beings has been demonstrated in the case of aminopterin only. At least three other compounds probably are more effective, but because of their mode of action (destruction of the embryo) suitable trials have not as yet been undertaken.

Although these compounds appear to represent the most effective measures now available for the physiologic control of conception, it is important to note that their mode of action, viz., destruction of the embryo, places them in a category of less than universal acceptability. This objection would not apply in some cultures, but it is complicated by the fact that there is some danger of undesirable side effects on the mother and danger of inciting anomalous development in the embryo if administration of the drugs is undertaken at too late a date in pregnancy. The actual extent of these dangers are as yet unknown, but at the present time they are sufficient to warn us that their use in the control of fertility must be preceded by more extensive trials than have as yet been made.

The third group of studies listed earlier, that of plant extractives, has for some years been regarded as extremely promising. Actually this optimism has been unwarranted since the supporting information has had very scant basis in scientific evidence. The two plants which have been given more than cursory study are Lithspermum (sev-

eral species) and Pisum sativum. In neither case is the existing evidence more than mildly suggestive that these plants contain a substance which has any specific effect upon reproduction. It is true that evidence has been reported that would indicate, if it is not properly evaluated, that each of these plants has an adverse effect upon fertility. These studies, however, involved the feeding to laboratory animals of crude plant material in such amounts that the prerequisites of an essential diet were extremely limited. Such experiments prove nothing with regard to the existence of substances which have specific effects upon fertility.

These remarks should not be construed as damning the possibility that some plants may contain compounds with antifertility effects. Too many reports, legendary as their origins may seem, are available to ignore the possibility that some truth may exist in one or more instances. The problem continues to be one of determining how these reports can be evaluated to the best advantage. In the case of Lithospermum, approximately $200,000 has been invested in research without providing us with definitive evidence of the existence of a substance which has a specific effect upon fertility. Perhaps another $200,000 or even $20,000 would provide an answer, but the existing evidence does not suggest that such is the case. At the present time it would seem reasonable to encourage studies in plant extractives but not to expect that they will provide compounds with magic effects.

This discussion may suggest a bleak outlook on the prospect of an early solution to the problem of controlling fertility by physiologic methods. If such is the case, this discussion has served a purpose. The problem is at least as difficult as that which confronts those who are concerned with the cause and cure of cancer. Indeed, it is in its entirety more difficult because there will be no real problem involved in applying the control of cancer. The battle will be barely more than engaged when some laboratory worker provides us with a method for the control of fertility. The chances are good that both cancer and fertility will ultimately be controlled by physiologic procedures and that the solution of either problem will have a direct bearing on the other. In each case we must look toward a far-reaching understanding of basic cellular mechanisms and the application of such knowledge to the specific problems.

The Medical Complaints of the Relatives of the Psychotic, the Alcoholic and the Epileptic

WALTER C. ALVAREZ, M.D.

IT MAY INTEREST geneticists to hear how a practicing physician, an internist concerned mainly with gastroenterology, became more interested in neuroses, minor psychoses, and variants of epilepsy and the heredity of these troubles than he was in diseased stomachs and bowels.

A mixture in a family of dipsomania, psychosis, mental retardation and lack of sex interest. In 1912 a woman came to consult me about her mentally retarded daughter. Chatting with the mother, I soon saw that she was mildly psychotic. I then sought out her beautiful and distinguished, but unmarried sister, and found that their father was an eminent man, and their six brothers fine, able men. Another brother, however, was dipsomanic, and the father's brother was a red-nosed town drunk. I suspected then that some defective genes that had wrecked the life of the father's brother had lessened the sexual interests of one daughter, had produced psychosis in another daughter, dipsomania in a son and mental retardation in a grandaughter.

Mental defects need not breed true. This family was one of the first of thousands, the experiences of which were to teach me over and over again something that I had never been taught in college, that when a hereditary taint—or whatever one wants to call it—goes down through a family, it commonly does not breed true. It can produce psychosis in one child and alcoholism, epilepsy, mental retardation, homosexuality, chronic invalidism, or hypochondriasis in others.

To be sure, there is a strong tendency for manic-depressive psychosis and schizophrenia to breed true, and there is a tendency for epilepsy to breed true, but one in four of the epileptics whose families I have studied carefully in the last five years had depressed relatives.

My impression is strong that if we will study our cases without any preconceptions or any desire to prove anything, and if we will record without reluctance or emotion what happened in families such as the thousand and more I have studied in recent years, we will learn many things that are not now yet recorded in the books. We will find cases in which the facts indicate that even schizophrenic and manic-depressive psychosis can come out of the same family. I know full well that men will say that my diagnoses were inaccurate; I am just saying how the uncorrected facts appeared.

A mixture in a family of schizophrenic and depressive psychoses. In one of my families, many of whose members I know, the old father, from his pictures, appeared to be a somewhat degenerated schizophrenic. His story is not that of a manic-depressive. Yet, of his children, 6 were definitely nice-looking and able manic-depressives. In the grandchildren and children I find schizophrenia, homosexuality, hysteria, and other troubles appearing again and again.

Similarly, in some of my eccentric families, epilepsy appears occasionally as if superimposed on much psychosis and alcoholism, while in other families, psychosis and alcoholism appear occasionally in what looks like a predominantly epileptic family. This is the way the problem looks to someone like me who has no genetic theories to support.

A mixture in a family of psychosis, myxedema and poor adjustment. In 1912

I studied all the members of a large and distinguished family, and again I learned much that started me to observing how a taint can produce several types of trouble as it affects a number of sibs. In this family an able but schizoid man married an intelligent woman. Among the children there was one who spent much of her life in mental hospitals with what looked like a mild schizophrenia. For years, she did not degenerate, and at times she was sensible and sweet.

A sister could not make up her mind about anything and so became a recluse. Another sister was a fat asexual person with myxedema, and a brother deserted his fine family to consort with laborers and low prostitutes. Others of the family were fine and able people.

In psychotic families there may be defects in the glands of internal secretion. These people were the first to make me see that in families with a mental taint there can be members with defective glands of internal secretion Such persons can have goiters, diabetes, and occasionally disease of the adrenal glands. Many of the women have a poor ovarian function. Many of the men have little sexual interest, and they can lose this by the time they are forty.

Without very careful genetic studies I cannot say that the incidence of diseases of the glands of internal secretion is higher among the psychotic than it is in the population as a whole. All I can say is that when a schizophrenic mother has two schizophrenic daughters and a third daughter with Addison's disease, one wonders if the adrenal gland, so closely related to the nervous system, might have been injured by the same defect that hurt the brain. I felt surer of this when, in a similar family, I found two cases of Cushing's tumor of the adrenals.

The minor equivalents of psychosis. In the years that followed 1912, I came to see that while students of psychosis had early recognized certain major equivalents of psychosis, such as alcoholism, mental retardation, unemployability, criminality, religious fanaticism and vagabondism, no one whose writings I ever encountered, had tried to identify the *minor* equivalents.

Recently, I analyzed the records of 574 of my patients who had had psychotic and/or alcoholic near-relatives. I listed their symptoms and tallied the number of them that had been observed. (I studied together the relatives of the psychotic and the alcoholic because I could not see any difference between them.) Among these 250 men and 324 women who had psychotic or alcoholic relatives, at least 37 per cent said they suffered from either definite depressions or milder "blue spells" or crying spells, or spells in which they experienced a "terrible" morning fatigue or great feelings of exhaustion, or toxicity.

Thirty-seven per cent of the 324 women had suffered from a hysterical type of nongaseous bloating of the abdomen. At least 35 per cent of the 574 "relatives" complained of great nervousness. Thirty-one per cent suffered from a poor adjustment to life. Thirty per cent were full of fears and anxieties. Ten per cent feared that they were going insane. At least 5 per cent were hypochondriacs, always certain that they had some awful disease such as heart trouble.

Twenty-five per cent suffered from short spells of diarrhea due to a panic of fear that they were going insane as a mother or an aunt had done.

Interesting was the finding that many of the "relatives" and particularly the relatives of schizophrenics, complained of weird and uncomfortable sensations. Schizoid persons complained of worms crawling under the skin, or coals of fire under the skin, or the gushing of blood or air in the head or thorax. At least 18 per cent complained of having lost their old good sense of balance. Seventeen per cent felt terribly tense, as if they would jump out of their skin or "fly to pieces." Seven per cent complained of numbness here and there

in the body. Many were terribly apprehensive. Many complained of weird spells in which their nerves played tricks on their several organs, such as the heart, bowel, arteries and skin.

Many had sexual troubles, and 22 per cent of the 324 women remained single. Many of those who had married had made such a poor adjustment that they had been divorced. Naturally, many of the schizoids were "asexual;" they could not love anyone, and had little interest in marriage.

Twenty-one per cent of the "relatives" were bad worriers. Interesting was the fact that 18 per cent complained primarily of pain. When this pain was in the abdomen, one in ten got operated on again and again. Some had from 5 to 7 operations—all without avail. Many others complained of aches and widespread and constant "miseries."

At least 15 per cent of the women had suffered from hysterical episodes. Many of the "relatives" vomited their breakfast. Fifteen per cent complained of nausea. Frank epilepsy was found in 14 per cent of the families tainted with psychoses and alcoholism. Thirteen per cent of the 574 "relatives" were constitutionally inadequate, and 12 per cent of the women complained of urinary frequency and distress of a neurotic type. Many suffered from vague indigestion. Many of both the men and women had sexual difficulties. There was frigidity, dyspareunia, and menstrual depression in the women, and premature ejaculation and loss of sexual interest and potency in the men.

At least 10 per cent of the "relatives" were schizoid and 9 per cent were cyclothymic.

The failure of physicians to diagnose psychosis. My experience with psychotic persons is that today these people can go through even a great university clinic, and many can come out without their real trouble being suspected or recognized. They will come out with either a diagnosis of some incidental finding, such as gallstones that are silent and harmless, or the statement that "there is nothing the matter."

A while ago, I saw a woman of 60 with an obvious agitated depression, so bad that my receptionist came to me and said there was an insane woman in the waiting room, and would I please see her right away as she was upsetting the other patients. This woman had twice been thoroughly examined in one of America's greatest institutions of medical learning. The first time she had had some silent gallstones removed and the second time her perineum had been repaired!

The great need for recognizing equivalents of psychosis. Obviously, there is a tremendous need today for a better training of physicians so that they will recognize psychoses and especially the minor equivalents of psychosis. They need, also, training in recognizing the first symptoms of insanity and the minor psychoses of childhood.

It should now be obvious that, until the minor equivalents and carrier states are commonly recognized by physicians and recorded on patients' records, studies on the inheritance of psychoses and epilepsy can have little volue. Too many of the patients' relatives who pass in the community as normal are anything but normal, as could easily be shown by any able psychiatrist or epileptologist.

The old and trusted family physician often knows that even an eminent and highly respected member of a community behaves insanely at home, or drinks over a pint of whiskey before retiring at night, or is so paranoid that he is always snooping on his wife, or is at times so depressed that he does little at his place of business.

A schizophrenic whose psychosis was said to come out of "a clear sky." I remember, years ago in California, having an argument with a psychiatrist friend of mine who was writing up for publication a case of what he called "isolated schizophrenia." He thought the young man's mental break-

down was a beautiful example of insanity that appears as an isolated example of mental disease in a "perfectly normal" family. He felt sure that the cause must have been a sexual trauma in childhood.

I had my doubts, and because my interest was piqued, I began some detective work. I looked up the old retired family doctor who, for 50 years, had taken care of the schizophrenic lad's mother, father and grandfather. The doctor told me that the grandfather had been an eccentric, mildly psychotic "old devil" who had managed to keep from most of his neighbors the fact that he was dipsomanic and a "lost weekender." His wife was eccentric, as was his daughter. His grand-daughter, the mother of the schizophrenic lad, in her youth was markedly schizoid, as was a sister.

These women had as cousins one who was mentally retarded, another who the old doctor maintained committed suicide, two who drank a bit, and a few who were not well adjusted.

In the ten years that followed the writing of the psychiatrist's paper, two of the schizophrenic lad's sisters developed enough mental trouble so that they placed themselves in the hands of a psychiatrist, and another developed a psychosis so bad she had to be committed. These extra 3 cases in the family should have upset my friend's theory as to the etiology of the disease he was studying. One wonders why 4 out of 5 children had so much mental trouble. It may have been because two carriers had married. A strain of schizophrenia and homosexuality ran through the lad's father's family. In his youth, this father was schizoid.

I think this story should show clearly enough that, without the sort of information that only an investigator on-the-spot can get, any genetic study must be valueless.

Hundreds of times, when my assistant wrote on a history sheet, "No insanity or epilepsy in the family," I soon drew from the patient or his mother or his old doctor a remarkable story of bad heredity, running through three generations.

The other day I read a statement made by an authority to the effect that the incidence of epilepsy among the children of an epileptic is 2 per cent. Dr. O.P. Kimball, who studied a series of epileptic families over the course of years, got a figure of about 37 per cent! He said he got much of his information only after years of effort, and after making friends with the parents of epileptic children.

In my study of 99 persons who had epileptic near-relatives, I found 59 who had definite signs of epilepsy—rarely grand mal, more often petit mal, and often a form of psychic epilepsy. If I could have gotten electroencephalograms in all cases, I probably would have found more persons with signs of epilepsy.

Some common equivalents of epilepsy. Among the 99 relatives of epileptics, I found that the incidence of many nervous symptoms was about the same as among the relatives of the psychotic and the alcoholic. Depressions, phobias, hysteria, aches and pains, compulsions and delusions and dazed feelings were found more often among the relatives of the psychotic and alcoholic.

Very interesting was the finding that irritability, irascibility and a bad temper were found 2 or 3 times more often among the relatives of the epileptic. They suffered much from alcoholism (15 per cent as compared with 7 per cent among the relatives of the psychotic and alcoholic). Noteworthy is the fact that 29 per cent of the relatives of the epileptic had suffered from depressions.

Sixteen per cent of the men complained of a distressingly premature ejaculation.

The great need today for studying carriers of nervous mental disease. I think one of the next big forward steps in medicine will involve a study of the carrier states. The world is full of carriers of the many hereditary diseases, and I doubt if genetic studies will be of much value until most of them are recognized fairly early in

life. More and more tests are being developed for the recognition of persons who some day will have diabetes, hypertension, primary anemia, lateral sclerosis, and other troubles. As every geneticist knows, one of the difficulties in his work is his inability often to tell how many of the children of a propositus will some day develop the expected diseases.

During my many years of practice, I have become impressed with the fact that, if we physicians would only observe well, if we would take better histories, and if we would know well the earliest symptoms of psychoses and epilepsy, we could recognize millions of carriers.

SUMMARY

A consultant in medicine has studied hundreds of families in which a taint which, in one member of the family produced psychosis, produced in others alcoholism, epilepsy, mental retardation and any one of many minor equivalents of psychosis. Evidently, mental difficulties do not always breed true.

Among minor equivalents of psychosis were found: blue or moody spells; crying spells; spells of great fatigue or exhaustion or feelings of toxicity; hysterical bloating of the abdomen; poor adjustment to life; many fears and anxieties; hypochondriasis; brief spells of diarrhea due to panics of fear; weird and uncomfortable sensations throughout the body; a loss of the sense of balance; numbness in several parts of the body; spells in which the nerves played tricks with several organs; sexual difficulties; a great tendency to worry; pains and aches all over; hysteria; epilepsy; constitutional inadequacy; severe pre-menstrual depression, and premature ejaculation in the men. Some of the "relatives" were schizoid; some were cyclothymic and some were alcoholic.

Today, psychotic persons go from clinic to clinic with their real difficulty often unrecognized.

The author describes a case of what was reported as an isolated case of schizophrenia in a perfectly "normal family." By interviewing the family doctor and local townsfolk, much psychosis and psychotic equivalents were found scattered throughout 4 generations.

It is shown that if one studies with great care the relatives of epileptics, one can find over 50 per cent who have minor forms of the disease. Very important in the future of medicine will be the study of the very common carrier states of diseases.

POPULATION PLANNING IN GREAT BRITAIN

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The Family Planning Association
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GREAT BRITAIN IS VERY FORTUNATE in having no laws against propaganda for birth control or the sale of contraceptives. This position was established nearly eighty years ago in 1877 when Charles Bradlaugh, Member of Parliament, and Annie Besant who was his co-worker in the Freethought Movement re-printed "The Fruits of Philosophy" by Dr. Charles Knowlton which was first published anonymously in New York in 1832. They did this deliberately because a printer and publisher in England who has re-issued the pamphlet the previous year had been successfully prosecuted.

Mr. Bradlaugh and Mrs. Besant were duly tried, the indictment accusing them

> "of unlawfully and wickedly devising, contriving, and intending, as much as in them lay, to vitiate and corrupt the morals as well of youth as of divers other subjects of the Queen, and to incite and encourage the said subjects to indecent, obscene, unnatural and immoral practices, and bring them to a state of wickedness, lewdness and debauchery. . . ."

Although the jury returned a verdict against the defendants this was quashed on appeal.

The following year, 1878, the Malthusian League was founded with Dr. Charles Drysdale as President and Mrs. Besant as Secretary. After years of propaganda this League eventually started a clinic at Walworth, London in September, 1921. A few months earlier Dr. Marie Stopes, whose books "Married Love" and "Wise Parenthood" had already met with great popular success, had founded the Society for Constructive Birth Control and had opened the first birth control in Britain.

Development after 1921 was more rapid. Several groups were started to open clinics to investigate methods, to undertake work abroad and to do propaganda. All of these combined in 1930 to form the National Birth Control Council, later called the Family Planning Association. Today all groups, except the Constructive Birth Control Society (President, Dr. Marie Stopes), are merged in the F.P.A.

The Council was exceedingly fortunate in its chief officers. The President, Lord Horder (then Sir Thomas Horder), was not only Physician to the King, but he was widely known and as widely respected by both the medical profession and the general public. Lady Denman, Chairman of the National Executive Committee, was wife of a former Governor General of Australia, daughter of a famous man of business (Lord Cowdray) and herself Chairman of the National Federation of Women's Institutes and as such known and loved up and down the country. These two not only governed the affairs of the Council wisely, but their support ensured attention from many official bodies and others who might have preferred to ignore the whole subject.

In Great Britain, elected bodies in towns and counties have responsibility for local Government within the framework of laws passed by Parliament. These local authorities, as they are called, function through Committees (e.g. for Health, Housing, Education, etc.) and salaried staff. They receive grants from Government Departments and also levy rates on local inhabitants.

Government Departments have no direct power over local authorities, but they interpret the law and make representations

and urge the local body to do this, that, or the other, and in the last resort, a Government Department can stop its financial grant if it considers the local authority is acting *ultra vires*. In practice, however, the Minister's word is usually accepted as law, and local authorities are almost overanxious to get the approval of whichever Ministry is concerned before it will undertake any course of action which has no precedent.

Up to 1930 the Ministry of Health had refused all requests to allow or encourage local health authorities to give any advice on birth control, but in April of that year a conference on the "giving of information on birth control by Public Health Authorities" was held in London and was attended by many representatives, both of those Authorities and of voluntary organizations. This conference called upon the Minister of Health "to recognise the desirability of making available medical information on methods of birth control to married women who need it." In the following July the Minister issued the first of several memoranda which, in effect, gave the authorities permission to provide advice on contraception for certain classes of women "for whom further pregnancy would be detrimental to health," but what in fact *was* detrimental to health was left entirely to the local Medical Officer of Health to decide. Some considered that it covered spacing of families, but at least one said that even an epileptic mother of eleven children was not eligible.

The newly formed National Birth Control Council devoted its main efforts to persuading Local Health Authorities to implement the Ministry's memorandum which was purely permissive, but quite early in its history a new development occurred. A local Health Authority at Plymouth in Devonshire offered to lend the Council premises to start a voluntary clinic. This plan had obvious advantages. The clinic could deal with all cases—not only those in medical need of advice—it would work under the aegis of the local authority, and it would have free premises. The plan was submitted to the Minister of Health who said his consent was not required, and in 1932 the first voluntary clinic to be held on the premises of a local health authority was started. Today there are 211 branch clinics of the Family Planning Association, and three quarters of them are held on the premises of either a hospital or a local health authority, mostly free of charge. In addition, many authorities either pay a block grant or a per capita fee to cover cases eligible for advice under the Ministry of Health memoranda who are officially referred to the F.P.A. clinic.

Although Headquarters makes an initial loan to new clinics, F.P.A. branches have to support themselves financially. Some of them are helped by voluntary subscriptions and donations, but the main sources of income are patients' fees and the sale of appliances and literature. Fees vary from clinic to clinic; a few have graduated scales and one or two have income limits but the normal fee is about 5s. and it is reduced or remitted altogether in cases of necessity. F.P.A. clinics are staffed by salaried doctors and nurses, and by lay workers, mainly unpaid.

Self-supporting clinics are only possible where the population is large enough to supply numbers of patients. In memory of Lady Denman, who was still Chairman when she died in 1954, a special fund is being raised to start clinics in rural areas so that countrywomen, to whose interests she devoted so much of her life, may be able to get the help they need.

In 1949 a Royal Commission on Population was appointed by the Government "to examine the facts relating to the present population trends in Great Britain; to investigate the causes of these trends and to consider their probable consequences; to consider what measures, if any, should be taken in the national interest to influence the future trend of population and to make recommendations." Its Report, issued five

years later, reached the conclusion that there is a steady increase of the use of family limitation by married couples. It said:

> "We agree with the view that there is nothing inherently wrong with the use of mechanical methods of contraception. Our survey of the history of family limitation leaves us no doubt that, if these methods were not available other means would be used, and some of them e.g. criminal abortion, the prevalence of which is even now distressingly high, are very undesirable. There is no prospect that men and women, having acquired control over the numbers of children they will have, will abandon it. Nor is it desirable that they should. The spread of contraceptive knowledge represents a big extension of man's control over his circumstances. As such it brings many problems with it. But it has been one of the conditions of the great social advances that have been made since the 19th century; it has made possible for increasing numbers of people the planning of the size of their families, and has helped to free women from excessive burdens and to ensure that more and more children born are wanted children. Control by men and women over the numbers of their children is one of the first conditions of their own and the community's welfare, and in our view mechanical and chemical methods of contraception have to be accepted as part of the modern means, however imperfect, by which it can be exercised."

and it also considered

> "that public policy should assume and seek to encourage the spread of voluntary parenthood."

Finally it recommended that

> "The giving of advice on contraception to married persons who want it should be accepted as a duty of the National Health Service and the existing restrictions on the giving of such advice by public authority clinics should be removed."

This report has never been discussed in Parliament, but its conclusions have certainly had an effect on public opinion.

The medical world in Great Britain has become much more cooperative as the years have passed, but provision for training doctors in contraceptive technique rarely exists in teaching hospitals. Some of these send their students to F.P.A. clinics, but most do little or nothing. Twenty-nine F.P.A. clinics have been inspected by the Association's medical panel and recognized as suitable to train doctors and nurses.

In Great Britain as in other countries there is a multiplicity of contraceptives varying greatly in efficiency, reliability and price. The F.P.A. has an Approved List of those contraceptives which pass certain laboratory tests and have proved harmless over many years of clinic use. Manufacturers whose goods are on the list have to conform to certain conditions of publicity and advertisement.

The other problem of family planning, *i.e.*, sub-fertility, has not received from the medical profession in Great Britain the attention it deserves, particularly in the field of male infertility, despite the fact that the Biological and Medical Committee of the Royal Commission of Population reported that

> "Of a typical group of women marrying at various ages in the proportions implied by 1938 marriage rates, probably between 5 per cent and 8 per cent on the average are likely to be involuntarily childless."

The Committee also said that

> "Few people are aware of the importance of the male factor in fecundity and there is still a general tendency to regard the wife as being 'to blame,' and for the husband to refuse to submit to examination."

In 1945, the F.P.A. established a seminological centre in London, and both here and at the F.P.A. branch in Exeter valuable pioneer work has been done. At the London center 20,000 cases have been dealt with, and medical journals have published accounts of its work. Many F.P.A. clinics are undertaking at least preliminary advice and have secured the cooperation of local hospitals and consultants.

Included in family planning work in this country is advice on cases of marital difficulties. The need for help often becomes apparent at a Family Planning clinic dur-

ing the course of the doctor's examination and questions. It is met in varying ways. A few clinics have special sessions, some refer cases to a local Marriage Guidance Council, a Consultant or another possible source of help, others deal with cases as they arise.

Most clinics give advice to couples before marriage and some have special sessions for men.

Over the years, the attitude of the British Broadcasting Company and of the British Press has been, with a few notable exceptions, one of neglect rather than hostility. Indeed there has been until recently what has amounted to a conspiracy of silence on the subject of birth control and the same has applied, in general, to public opinion.

But during the last few months there has been marked evidence that family planning is moving from the realm of propaganda into that of progressive news and discussion. Seven months ago the then Minister of Health, Mr. Ian Macleod, M.P., to mark the Silver Jubilee of the Association, accepted an invitation to visit a branch clinic (North Kensington Marriage Welfare Centre) and the Headquarters of the F.P.A. where he met Officers and Committee Members with some of whom he afterwards lunched. This event led to an appearance on B.B.C. Television, a talk in the B.B.C. programme "Woman's Hour", a leading article in the London *Times,* another in the *British Medical Journal* and reports in countless other newspapers and journals.

As regards the attitude of organized religion in Great Britain, the Roman Catholic Church opposes the establishment of F.P.A. clinics and has occasionally succeeded in persuading a local health authority not to grant the use of premises. There is nothing in the doctrine of the Church of England or of the Free Churches to preclude the use of mechanical methods of contraception, and individual Bishops, Free Church leaders and ministers of different denominations are active supporters of the F.P.A. The Secretary of the Department of Christian Citizenship of the Methodist Church has been officially appointed to the Executive Committee of the F.P.A., and it is possible that the Baptist Union may take similar action.

To sum up: in Great Britain today the Family Planning Association is opening new clinics at the rate of one every eleven days. Last year 250,000 women and men visited F.P.A. clinics; in addition there are independent clinics, notably that run by Dr. Marie Stopes in London. Public interest and support, both official and unofficial, are growing steadily. The present position and probably future development were well expressed by the *British Medical Journal* a few months ago.

> "The history of the Family Planning Association as a voluntary organisation follows the usual British pattern—the demonstration of a social or medical need followed by attempts to meet it, growing public favour, and expansion of staff and premises leading to the national provision of services. The natural conclusion—full official recognition, followed by the assumption of the work, or at least financial responsibility for it, by the State or by local authorities—does not seem likely while birth control remains a subject of controversy."

METHODOLOGICAL LESSONS OF THE INDIANAPOLIS FERTILITY STUDY*

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A RECENT ISSUE of *Eugenics Quarterly* contained a brief description of a contemplated new study of factors affecting fertility in urban areas of the United States.[1] That article indicated that the interest in a new study is "an outgrowth of the now virtually completed Indianapolis Study." Although it described the plans for the new study, it gave little explicit attention to the methodological lessons derived from the Indianapolis Study.[2]

Actually the "lessons" of the Indianapolis Study have had considerable bearing on the shaping of plans for the new study and and also on studies of fertility in other areas. It therefore seems appropriate, and of possible value to students, to have a brief but systematic presentation of the methodological lessons of the Indianapolis Study. Four broad types of "lessons" are discussed: (a) those concerning the independent variables; (b) those concerning the dependent variables; (c) those concerning the size and character of the sample; and (d) those concerning the general design of the Study.

1. *The Independent Variables: Social and Psychological Factors.*

(a) In a study entitled "Social and Psychological Factors Affecting Fertility" the social and psychological factors were, in the nature of the case, assumed to be the independent variables and fertility behavior was considered to be the dependent variable. It was realized from the beginning, of course, that in social science there is an interaction of variables and this matter will be discussed later. For the present, the chief lesson to be emphasized concerns the generally closer relationship of fertility to socio-economic status than to the psychological factors. The relation of fertility and fertility-planning status to socio-economic status was rather consistently sharp for a variety of measures of socio-economic status. The observed relation of fertility behavior to most of the psychological characteristics considered was generally much less pronounced or less regular, *especially when socio-economic status was held constant.*

*This paper was presented in substantially its present form at the World Population Conference, held in Rome in September, 1954.

[1]Charles F. Westoff, Elliot G. Mishler, Robert G. Potter, Jr., and Clyde V. Kiser: A New Study of American Fertility. *Eugenics Quarterly.* Vol. 2, No. 4, December, 1955, pp. 229-233.

[2]The Indianapolis Study has been in the hands of a Committee on Social and Psychological Factors Affecting Fertility consisting of the following: Lowell J. Reed (Chairman), Daniel Katz, E. Lowell Kelly, Clyde V. Kiser, Frank Lorimer, Frank W. Notestein, Frederick Osborn, S. A. Switzer, Warren S. Thompson, and P. K. Whelpton. Collaborators in the analysis of the data have consisted of Robert B. Reed, Ronald Freedman, Lee F. Herrera, Jeanne E. Clare, J. F. Kantner, Charles F. Westoff, Marianne DeGraff Swain, Nathalie Schacter, Ruth Riemer, Lois V. Pratt, Edgar F. Borgatta, Robert G. Potter, Jr., Erwin S. Solomon, and H. V. Muhsam.

The analytical reports on the Indianapolis Study have been published in the Milbank Memorial Fund *Quarterly.* The following four volumes of reprints have been bound. (A fifth volume will complete the series):

P. K. Whelpton and Clyde V. Kiser (Editors): Social and Psychological Factors Affecting Fertility.

Vol. One: The Household Survey in Indianapolis (Parts I-III), 1946, pp. 1-138.

Vol. Two: The Intensive Study: Purpose, Scope, Methods and Partial Results (Parts IV-X), 1950, pp. 139-466.

Vol. Three: Further Reports on Hypotheses in the Indianapolis Study (Parts XI-XVII), 1952, pp. 467-800.

Vol. Four: Further Reports on Hypotheses and Other Data in The Indianapolis Study (Parts XVIII-XXIV), 1954, pp. 801-1086.

There was one outstanding exception. The relation of economic security to the size of completely planned families was conspicuous and quite independent of socio-economic status. Nevertheless, the central implication here is the importance of securing a better understanding of the factors underlying differential fertility according to socio-economic status and especially the role of psychological factors.

(b) As indicated in previous critiques, the scope of the Indianapolis Study was too wide for adequate treatment. There were twenty-three hypotheses, each of which was concerned with the relation of a given social or psychological factor to effectiveness of contraceptive practice and size of planned family. Thus when we say that the scope of the Indianapolis Study was too broad we mean that too many social and psychological factors were included for investigation.

(c) A closely related criticism is that the twenty-three hypotheses of the Indianapolis Study were not bound together by an integrating theory or organizing principle. As indicated in a previous publication, the twenty-three hypotheses which form the basis of the Study are not systematically interrelated and little attempt was made to link the hypotheses to any basic social or psychological theory.[3]

(d) As a result of attempting to investigate too many hypotheses in one study, the variables were not always precisely defined or well conceptualized. An example of this was the variable "personal interest in children." The analysts correctly interpreted this variable as meaning "ego-centered interest in children" but the concept was incorporated in the hypotheses without any precise definition.

(e) A related consequence of attempting a study too broad in scope and of insufficient time devoted to conceptualization was that the variables in the hypotheses—especially the psychological variables—were not always measured. The time spent on field trials evoked the admiration of contemporaries, but insufficient attention was paid to tests of reliability and validity.

Preliminary experimentation with data relating to "economic tension" (difference between the actual and desired levels of living), might have sounded a warning about the selective factors in these particular data. "The selective factor consisted in an inverse relation of what we call 'economic tension' to socio-economic status. That is, couples of lowest socio-economic status tended to exhibit the largest percentage difference between what they had and what they wanted."[4]

By way of extenuation it may be said that when the committee decided to attempt to test twenty-three hypotheses, they regarded their effort not as an attempt to get an adequate testing of each hypothesis but rather as a "dragnet" approach. It was hoped that the "dragnet" would serve to indicate which of the twenty-three hypotheses could be discarded and which ones deserved more intensive study in future investigations.

2. *The Dependent Variables: Proportion of Couples Practicing Contraception Effectively and Size of Planned Families.*

As stated in most of the hypotheses of the Indianapolis Study the two dependent variables were "proportion of couples practicing contraception effectively and size of the planned family." This design worked rather nicely in that the families classified as "practicing contraception effectively" were synonomous with those classified as "planned families." Both groups were composed of couples classified either as "number and spacing planned" or "number planned."

In a paper prepared for this World Population Conference (advance copy of which

[3] *See* Clyde V. Kiser: Exploration of Possibilities for New Studies of Factors Affecting Size of Family. A report on a Round Table held in connection with the 1952 Annual Conference of the Millbank Memorial Fund. The Milbank Memorial Fund *Quarterly*, xxxl, No. 4, October, 1953, p. 444.

[4] Clyde V. Kiser: The Indianapolis Study—An example of Planned Observational Research. *Public Opinion Quarterly*, XVII, No. 4 Winter, 1953-54, p. 508.

was kindly sent to the present writer), Dr. Robert M. Dinkel questioned the adequacy of the classification by fertility-planning status that was used in the Indianapolis Study. It is true that in the analyses chief reliance has been placed upon single indices of fertility-planning status and fertility, *i.e.*, classifications relating to the total period since marriage. More detailed indices were available (such as regularity of contraceptive practice at the time of each pregnancy), but the sample is too small to warrant use of the more detailed indices. Certainly, it would be well to have sufficient data to relate psychological characteristics to fertility-planning status during specific periods of married life or during specific interpregnancy intervals.

Likewise, although data were collected in the Indianapolis Study regarding specific births, the hypotheses themselves, as indicated above, related to total "size of planned families." The failure to secure closer relations between psychological factors and fertility suggested that such relationships may have been blurred by the use of total fertility rather than specific pregnancies or pregnancy intervals. The implication was that, at least as an experiment, a future study might focus on factors associated with the occurrence and timing of one or more specific pregnancies.

3. *Size and Character of the Sample.*

(a) The Indianapolis Study sample has frequently proved to be too small to afford good tests of the hypotheses considered. A reduction in scope of the study should make it possible to have not only better data but also larger samples.

The preceding discussion of studying both fertility-planning status and fertility is relevant here. If a study is concerned only with investigating the fertility of planned families, there is a greater opportunity to expand the sample with which one works.

(b) Despite the large number and diverse nature of the hypotheses in the Indianapolis Study, there was no variation, by hypothesis, in the basic eligibility requirements for couples in the Study. As a consequence, whereas the sampling scheme was admirably suited to some of the hypotheses, it was ill-suited to others. For example, one of the hypotheses is concerned with the relation of migration and rural-urban residence since marriage to fertility. Yet one of the requirements for eligibility in the Study was residence in a large city most of the time since marriage. The specific lessons to be learned here are that (1) the sampling scheme should be fitted to the hypotheses for study; (2) that the next efforts be directed toward several small studies rather than toward one large study; and (3) that the relevance of such factors as migration to fertility can be studied more effectively with census data than with studies of the Indianapolis type which are expensive and of necessity based upon small numbers.

(c) The Indianapolis Study was restricted to couples having the following characteristics: husband and wife native white, Protestant, with at least a complete grammar-school education; married during 1927-1929; wife under 30 and husband under 40 at marriage; neither spouse previously married; and residence in a large city (25,000+) most of the time (8+ years) since marriage.

There are both advantages and disadvantages in having a homogeneous group for study. The advantage is that homogeneity of the type secured prevents the necessity of splintering the analysis by such factors as color, nativity, rural-urban status, age, etc. The disadvantage is that the use of the homogeneous sample removes one from the actual conditions. It may be stated, however, that the use of the homogeneous sample is usually more justified in studies concerned with casual relationships than in studies designed to ascertain the frequency of given characteristics in the total population or in efforts to translate rates observed for the study sample to rates

for the total community.

Another disadvantage is that restriction to a homogeneous sample probably tends to narrow the range of variation of both the independent and dependent variables under consideration. This may be an important reason why the Indianapolis Study did not exhibit stronger relations between psychological variables and fertility. By restriction to a highly homogeneous group like that in the Indianapolis Study and then by making the sample further homogeneous by restriction to planned families and still more homogeneous by holding socio-economic status constant, one doubtless reduces very much the range of variations in both the psychological characteristics and the fertility of planned families. When one further considers that our measures of psychological characteristics probably were too crude to afford precise differentiations, it may be little wonder that the Study failed to indicate strong and consistent relations of fertility behavior to psychological characteristics.

4. *General Design of Study.*

(a) Perhaps closely related to the methodological problems inherent in the homogeneous sample are those inherent in the atomistic approach, *i.e.*, the separate analysis of the variable under each hypothesis. In a very real way this approach necessitates the assumption that all other factors are equal when groups are classified on the basis of only one variable at a time. The atomistic approach neglects the sociological and psychological axiom that motivations are multiple and complex. It is true that some attempt has been made to investigate the simultaneous impact of several variables,[5] but the Study was structured and the results were analyzed largely in terms of twenty-three separate hypotheses.

(b) The attempt to study social and psychological factors affecting fertility by the actual collection of data at only one point in the married life (12–15 years after marriage), raises a variety of problems. First of all is the question of memory. To what extent can the person remember the details regarding, say, contraceptive practice since marriage, attitudes toward successive pregnancies, and attitudes of the past 12–15 years about a variety of other things? In the second place, the dependence on retrospective or *ex post facto* data frequently makes it difficult to differentiate between cause and effect. This deficiency was illustrated particularly by the data regarding preferences as to the sex of children, by the relation of economic security to fertility among "excess fertility" couples, and by the data on "economic tension."

(c) An indicated lesson of the previous condition is the need for a study that will utilize changes in time as the functional units of classification. A longitudinal study of some type is rather clearly indicated. If a study can "catch" couples before they are married or before they have a child of a given order, it has a good chance to overcome some of the problems associated with the use of retrospective data.

It is recognized that the orthodox longitudinal study is expensive in money and time. It is also acknowledged that the study itself may exert an influence on the fertility behavior of some of the members of the panel under study. Furthermore, past investigations have indicated that the "dropouts", *i.e.*, those withdrawing from the study, are a selected group.[6]

However, there are ways and means of

[5]Charles F. Westoff and Clyde V. Kiser: Social and Psychological Factors Affecting Fertility XXI. An Empirical Re-examination and Intercorrelation of Selected Hypothesis Factors. The Milbank Memorial Fund *Quarterly*, October, 1953, xxxl, No. 4, pp. 421-435. (Reprint pp. 953-967.)

Edgar F. Borgatta and Charles F. Westoff: Social and Psychological Factors Affecting Fertility XXV. The Prediction of Total Fertility. The Milbank Memorial Fund *Quarterly*, Vol. 22, No. 4, October, 1954, pp. 383-419. (Reprint pp. 1087-1124.)

Charles F. Westoff and Edgar F. Borgatta: Social and Psychological Factors Affecting Fertility XXVI. The Prediction of Planned Fertility. The Milbank Memorial Fund *Quarterly*, Vol. 23, No. 1, January, 1955, pp. 50-62. (Reprint pp. 1125-1137.)

[6]Jean Downes: The Longitudinal Study of Families as a Method of Research. The Milbank Memorial Fund *Quarterly*, April, 1952, xxx, No. 2, pp. 101-118.

at least partially overcoming some of the selective features of a longitudinal study. Students are experimenting with partially longitudinal studies in an effort to offset some of the objectionable features of the full-blown longitudinal study.

(5) *Application of the Methodological Lessons.* The methodological lessons of the Indianapolis Study are being applied, to an important extent, in the plans for the new study mentioned at the outset of this article. As described in an earlier issue of this journal, the contemplated study probably will be a partially longitudinal study. Its basic purpose will be to ascertain the social and psychological factors related to the occurrence and timing of third births among selected couples who recently had second births. Instead of being restricted to homogeneous couples in a single city the new study probably will be representative of native-white two-parity couples in a substantial segment of the urban population.

Finally, although the Indianapolis Study may be regarded as appropriate only for the low fertility countries, many of the questions asked in that Study have been used in fertility studies in Puerto Rico, India and other places. Although completely planned families are rare in such areas, it is becoming increasingly important to study the bearing of values, customs, and attitudes on practices regarding marriage and the family in agrarian societies. It is hoped that through a widening of experience and through an interchange of ideas at conferences such as the present one, studies of social and psychological factors affecting fertility might be substantially developed during the next twenty-five years.

BIOLOGY, RATIONALITY, AND FERTILITY
A Footnote to Transition Theory

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PERHAPS FEW DISCIPLINES ARE as modestly conscious of their limitations while as methodologically sophisticated as demography in its current phase of critical self-examination. Its most important theoretical statements are carefully hedged with restrictions and qualifications, and the tentativeness with which they are offered is matched by an urgency in the acquisition of new data and new knowledge. And yet modesty is self-defeating if it results in an avoidance of precision which makes theories difficult to confirm or disprove. A case in point is the body of generalizations which has become known as the "theory of demographic transition." This statement of the dynamic relations between declining mortality and fertility in the process of modernization is perhaps demography's best claim to general theoretical development,[1] yet the controversy over the validity of these generalizations has provided evidence that there is little agreement either on the definition of the variables, the precise nature of their dynamic relationship, or the predictions implied.[2] This situation may be regarded as unfortunate, even if traceable to an acute sense of the theory's limitations on the part of its principal expositors.[3]

We may ask first of all what sort of theory we are dealing with—is it an empirical theory, or is it a frame of reference? If it is the former, it must account directly for the observed variance in mortality and fertility. If it is the latter, it must tell us what categories of variables are important, and how we should relate them to one another. We can perhaps see the consequences of these alternatives most clearly in the case of fertility.

The Fertility Transition

Fertility has been a major focus of discussion. The problem of operationally defining it is discussed in a recent paper by Ryder.[4] Its status as a variable remains controversial. The prospective critic of the transition theory finds himself faced with the task of resolving this question before he can even attempt a rebuttal of the theory. For example, does the theory actually predict "incipient decline" produced by fertility below replacement levels in those countries which have already passed through the stage of "transitional growth?" What are the postulates which imply this predic-

[1]This is the conclusion of Rupert B. Vance in "Is Theory for Demographers?" *Social Forces*, 31 (October, 1952), pp. 9-13.

[2]See, for example, Joseph S. Davis, "Our Charged Population Outlook and Its Significance," *American Economic Review*, 42 (June, 1952), pp. 304-325; "Population and Resources," *Journal of the American Statistical Association*, 45 (September, 1950), pp. 346-349; "The Population Upsurge and the American Economy, 1945-80," *Journal of Political Economy*, 61 (October, 1953), pp. 369-388; Paul K. Hatt, Nellie Louise Farr, and Eugene Weinstein, "Types of Population Balance," *American Sociological Review*, 20 (February, 1955), pp. 14-21; Leighton van Nort and Bertram P. Karon, "Demographic Transition Re-examined," *American Sociological Review*, 20 (October, 1955), pp. 523-527; and Weinstein's "Comment" on the latter and "Reply" by the authors, *American Sociological Review*, 21 (June, 1956), pp. 369-373.

[3]Notestein, for example, in his most recent published statement of the theory, writes: "At present we cannot either list all the factors involved or attach precise weights to the factors we can list." Frank W. Notestein, "The Economics of Population and Food Supplies," *Proceedings of the Eighth International Conference of Agricultural Economists*, London: Oxford University Press, 1953, p. 18.

[4]Norman B. Ryder, "Problems of Trend Determination During a Transition in Fertility," *Milbank Memorial Fund Quarterly*, 34 (January, 1956), pp. 5-21.

tion? Or is "incipient decline" simply a somewhat misleading but inessential label for the "mature" balance between low fertility and low mortality?

The transition in fertility described by the theory may be interpreted in either of two ways: (1) It may be treated as a simple statement of the empirical relationship between fertility and some other essentially unitary variable such as "modernization" which might, in principle, be represented by an index number.[5] Or (2) it may be regarded as a description of the relations among *categories* of variables, that is, relations among more specific bodies of theory whose parameters can be obtained from direct observation. In this case it is a *frame of reference,* not a simple statement of an empirical relationship.

In the first interpretation, we are dealing with a theory from which we should be able to obtain a predicted level of fertility by specifying the observed values of the parameters. A test can be straightforward and direct. For example, can the theory predict the "baby boom," or tell us just how rapidly populations in "transitional" areas will grow? On the other hand, *frame of reference cannot be either confirmed or disproven from direct observation alone.* Only when the subordinate postulates are operationalized does the "transition" frame of reference become accessible to test. If we adopt the second interpretation, it follows that criticism of the theory for failing to supply specific predictions is wide of the mark. We wish to suggest a formulation involving the latter interpretation.

A Possible Formulation

Our proposed formulation can be put very simply and crudely as follows: the transition from "high" to "low" levels of fertility represents, in first approximation, a transition from a *biological* model of fertility to an *economic* model of fertility. By a *biological* model of fertility we mean the ideal-type situation in which levels of fertility are determined by the more or less direct operation of biological factors, conditioned by a set of social and psychological factors specific to a pre-industrial society. By an *economic* model of fertility we mean the ideal-type situation in which levels of fertility are determined by decisions based on the rational allocation of resources among competing wants of the type normally denoted economic, conditioned by a set of social and psychological factors specific to a modern industrial society. The transition in fertility represents, in terms of this particular formulation, the gradual limiting of biological determinants of fertility by a process of rational decision-making.

This formulation is scarcely unconventional, for it leads to the sorts of questions which have characteristically been asked by "transition"-oriented demographers. These are questions such as the following:

Countries in the stage of "High Growth Potential."[6] Since fertility is seldom or never observed at the biological maxima, even in pre-industrial societies, (a) what are the levels of fertility which would be expected on the basis of purely biological factors, and (b) what are the social and psychological factors which serve to reduce observed levels of fertility below these maxima? What are the magnitudes of the reductions induced by specific factors within the framework of such societies?

Countries in the stage of "Transitional Growth." To what extent can the decline in fertility in "transitional" countries be attributed to the gradual substitution of rational decision-making of the type broadly denominated "economic" for the direct operation of biological factors within mar-

[5]A partial example of such an approach, apparently involving also an interpretation of the theory of demographic transition as a static typology, is to be found in Hatt, Farr, and Weinstein, *op. cit.*

[6]The descriptive labels for the three types of countries are those employed by Notestein. See Frank W. Notestein, "The Population of the World in the Year 2000," *Journal of the American Statistical Association,* 45 (September, 1950), p. 335.

riage? What is the relation of improvements in the technology of fertility control to the spread of such a rational orientation?

Countries in the stage of "Incipient Decline." To what extent can fertility in highly developed industrial countries be explained and predicted in terms of purely "rational" economic decisions about childbearing, together with deviations from such decisions resulting from inefficiencies in the available technology of fertility control and its application within the family? When the variance attributable to these factors is subtracted, what is the magnitude of the residual variance which must be explained in social and psychological terms?

Some Needed Research

There is nothing very new or startling about this list of questions, except insofar as it suggests one critical gap in current work on fertility: the absence of any rigorous theorizing about the levels of fertility in the transitional and mature stages which would be implied by the operation of purely "economic" factors. To what extent does family investment in children follow the principles governing investment in other durable goods involving future costs which are relatively fixed and predictable? To what extent are children economically substitutable with such other commodities, and what sort of substitution effects result from variations in relative costs? What levels of fertility would be predicted on the basis of variations in (a) expected costs of child-maintenance, and (b) expected income? Could some portion of the post-war rise in fertility be attributed to changes in income expectations, and in the prices of commodities competitive with children? These questions are of course supplementary to the currently more prominent question of the direct costs of child-bearing, but they may be far more important than the latter.

There is ample evidence of interest in the refinement of the biological model of fertility, but notably little interest displayed in developing an adequate economic model. Almost all the other subordinate postulates incorporated in the "transition" frame of reference have been more thoroughly explored, and some further information on several will be provided by the "New Study of American Fertility."[7] Some information on the economic variables will probably also be forthcoming. Westoff, Mishler, Potter, and Kiser note in their recent article on the design of the new study that

> a fertility choice (whether implicit or explicit) involves a "cost" for the individual and the family, that is, certain desires and interests are either yielded or compromised in preference to others. (Decisions not to have children are as "costly," in a sense, as decisions to have them.)[8]

Mishler and Westoff, in a more technical paper, indicate interest in the process of family decision-making and in gathering some economic data.[9] Thus some information potentially useful for the construction of an adequate economic model probably will be collected. However, as Hauser pointed out in a discussion of the study design, relatively little attention will be given to the relation of the family decision-making process to the "general social-economic climate."[10] And perhaps equally as important is the exploration of the implications of what is already known about general economic behavior for the analysis of fertility under conditions of substantially complete voluntary control.

If the foregoing comments have any validity, it follows that a great deal of theorizing about fertility remains to be done if the transition theory is to be stated in testable or operational form. If a "transi-

[7]See Charles F. Westoff, Elliot G. Mishler, Robert G. Potter, Jr., and Clyde V. Kiser, "A New Study of American Fertility—Social and Psychological Factors," *Eugenics Quarterly*, 2 (December, 1955), pp. 229-233.

[8]*Ibid.*, p. 232.

[9]Elliot G. Mishler and Charles F. Westoff, "A Proposal for Research on Social Psychological Factors Affecting Fertility: Concepts and Hypotheses," *Current Research in Human Fertility*, New York: Milbank Memorial Fund, 1955, pp. 145-149.

[10]Philip M. Hauser, "Some Observations on Method and Study Design," *ibid.*, p. 155.

tion" frame of reference cannot at present be either confirmed or disproven, it *is* open to criticism if it fails to provide us with relative weights for the categories of variables it relates to one another. At present, the system of ideas known as the transition theory is perhaps most accurately described as a shrewd selection of materials from which to construct a theory. The particular fertility formulation discussed above is one of many possible ways of relating some of these factors to one another. We may expect a variety of others, for even in its present embryonic form the transition theory is a fruitful guide for observation and analysis.

The Obstetrician and Congenital Malformations in Brothers and Sisters

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WHEN A CONGENITALLY malformed infant is born, the parents usually ask their obstetrician what their chances are of having another defective child. This is a natural question, since brothers and sisters frequently resemble each other.

Obstetricians are likely to be at a loss to answer this question for a number of reasons. Congenital defects are of relatively infrequent occurrence. Text-books of obstetrics do not discuss the familial occurrence of congenital defects. The information that is available upon the subject is scattered widely throughout the literature. Two books: *Medical Genetics* by Arnold Sorsby and *Human Genetics* by R. Ruggles Gates contain a considerable amount of information dealing with congenital defects, but even these volumes may not supply the information desired by the obstetrician for the defect in question.

Data useful to the obstetrician on the likelihood of recurrence of certain congenital malformations in siblings has been summarized recently by Anderson (Table I). He stated that his figures were based upon

TABLE I

RISK FIGURES FOR LATER SIBLINGS

	Incidence in Population	*Risk Figures for Later Siblings*
All malformations	1 in 65	1 in 20
Central nervous system malformations (35%)		
Anencephalus	1 in 450	1 in 50
Spina bifida	1 in 375 } 1 in 200	1 in 25 } 1 in 22
Hydrocephalus	1 in 550	1 in 60
Mongolism	1 in 600	1 in 20
Muscular-skeletal malformations (25%)		
Harelip with or without cleft palate	1 in 1000	1 in 7
Cleft palate alone	1 in 2500	1 in 7
Polydactylia	1 in 1200	1 in 2
Syndactylia	1 in 2000	1 in 2
Clubfoot	1 in 1000	1 in 30
Malformed arms, hands	1 in 5000	—
Achondroplasia	1 in 7000	—
Congenital hip dislocation	1 in 1500	1 in 20
Cardiovascular malformations (20%)		
All congenital hearts	1 in 150	1 in 50*
Patent ductus	1 in 2500	1 in 50
Genitourinary malformations (6%)		
Hypospadias	1 in 1000	1 in 50 (?)
Polycystic kidney (infant)	1 in 15000	1 in 4
Gastrointestinal malformations (3%)		
Exomphalos	1 in 4000	less than 1 in 100
Diaphragmatic hernia	1 in 10000	—
Tracheo-esophageal fistula	1 in 6000	less than 1 in 100
Atresia ani	1 in 5000	less than 1 in 100
Multiple malformations (11%)		
Miscellaneous		
Pyloric stenosis	1 in 350	1 in 17

*For any type of heart defect to recur.

Courtesy of R. C. Anderson and S. C. Reed, *Journal-Lancet* (Minn.) 74:175, 1954.

a large number of reports and emphasized the fact that they were only approximate. It is assumed that all of the conditions which he studied arise from hereditary factors.

On the basis of the data assembled by Anderson, there would seem to be but little doubt that the congenital defects which he studied are quite likely to be observed in a second sibling, provided as has been said that the cause of the defect in the first child was on an hereditary basis. The chance that any of these hereditary defects will reappear in a subsequent sibling seems to be extremely great. It would follow from this that the parents of a malformed child exhibiting the malformation reported by Anderson would be taking a considerable risk by having more children.

The chance that a defect will appear in a sibling depends upon a number of factors.

In order to supply the parents with the best possible information, the cause of the defect should be determined first of all. If the malformation resulted from some influence which was not hereditary, such as maternal pelvic irradiation or German measles, no subsequent child should be abnormally developed, provided the same cause does not operate again.

If no known cause was responsible for the malformation, then it is very likely to be the result of hereditary influences. In such a case, a human geneticist should study the distribution of the defect among the blood relatives of the infant in question. Unfortunately, there are very few geneticists in this country to offer family counseling. If a geneticist cannot study the family, it may fall to the obstetrician to advise the family. Even if information is not available upon the hereditary character of the defect in question, it is possible to tell the family how hereditary defects, in general, behave with respect to their recurrence in brothers and sisters.

One study of a large series of families (1), into each of which a seriously defective child was born, and in which there was no evidence to implicate a known cause, has supplied some observations bearing upon the frequency with which serious congenital malformations appear in subsequent siblings. That study led to the following observations:

1. The parents had another congenitally defective child more often than would have been expected by chance.

2. The subsequent defective child had the same defect as the one observed in its earlier born sibling more often than would have been expected by chance, *i.e.*, in approximately 50 per cent of cases.

3. The second malformed child was more likely to be born next but one to the first defective child, than it was to be the next child after the birth of the first defective sibling.

4. The congenital defects appeared with increasing frequency after the mothers had passed 30 years of age.

On the assumption that the above observations are true, and if the family cannot be studied by a geneticist, what can the obstetrician say to the parents of the congenitally malformed child regarding having more offspring? He should give them all available facts. They should be the ones to make the final decision, as to whether they will attempt to have any more children.

The obstetrician's advice will have to depend upon: (1) Knowledge regarding the biology of congenital malformations as a group, and (2) the conditions peculiar to the family.

Certain serious malformations terminate the life of the afflicted individual relatively shortly after birth. If such a defect occurs, it is solely a problem of the parents, and may not even be a problem to them for a very long time. If the defect does not influence life expectancy, the malformation will be a problem, not only for the parents, but also for the affected individual.

The study mentioned above indicated that if two siblings carry a malformation,

the defect in the second sibling is quite likely to be the same as the one observed in the previously born defective brother or sister. If the defect seen in the first child accounted for its death shortly after birth, the parents may be willing to take the chance of having another child knowing that if it is defective, it is likely to have the same defect as the first child, and therefore it also is not likely to live very long.

The parental urge to possess at least one normal child is too well known to be mentioned. If the parents have at least one healthy offspring, this may be sufficient to satisfy the parental urge. In such a case, it might be best if they refrain from taking any further chances. On the other hand, if they have no living offspring, there might be more reason for them to accept the risk that another child will be affected, hoping that it will be normal.

The ages of the parents should be taken into consideration by the obstetrician in giving them advice. If they are under 30 years of age, there is less chance that they will have another defective child, than if one or the other parent is well over 30 years of age. After that time, there appears to be a steadily increasing chance that another child will be defective. However, regardless of the ages of the parents, they are still more likely to have another malformed child than would be expected by chance.

Although there appears to be a tendency for the malformation rate to increase with parental age, at the same time there is a small amount of evidence to suggest that the child born next after the defective sibling is less likely to be defective than is the child who is next but one to the first defective brother or sister.

On the basis of what is known regarding malformations in general, and without the benefit of a study of the family by a geneticist, what general advice can the obstetrician give to the family in question?

His first statement should be that the couple has a greater chance of having another congenitally malformed child than would be expected by chance. The study forming the basis for this statement indicated that the chance of the first child being defective was 1 in 200, whereas the chance that a sibling would also be defective was 1 in 8. In other words, the chance that another sibling would be defective was 25 times greater than the chance that the first one would be defective. These data might also be stated differently by saying that the couple still has 7 chances in 8 of having a normally developed child. This is a more optimistic way of looking at the situation.

If the couple has at least one normal child, it would seem best that they do not tempt fate by continuing to increase the size of their family. If either husband or wife is late in his or her reproductive life, this would be another reason why it would again seem best not to tempt fate by having more children.

If the couple has no living offspring, and both husband and wife are under 30 years of age, they might be advised to have another child. If the next child is normal, it would seem best not to tempt fate again by trying to have a third child. If the second child is defective, again it would seem best to end the family at that point. There is evidence to indicate that the risk of having a third malformed child is even greater than the risk of having a second malformed child.

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LETTERS

EDITOR'S NOTE:

Comment on The Eugenic Hypothesis has been published in EUGENICS QUARTERLY, *September 1955, December 1955, and March 1956. The following letter continues this discussion. Copies of "The Eugenic Hypothesis" are available upon request to the editor.*

To the Editor:

The eugenics movement is at the present moment largely utopian. It is animated by a dream of basic changes in the hereditary composition of the population. It would set up, in harmony with democratic principles of free individual choice, a scientifically sound scheme of birth distribution that would reverse what is believed to be present adverse selection. Its scientific orientation frees it from the dogmatisms, myths and emotional excesses that usually give momentum to socially reconstructive movements. This approach, however, may prove a serious handicap unless scientific mentality triumphs over tradition, authority and passion as foundations of the social values affecting marriage and family size. Success depends on three achievements: first, continued expansion of the rapidly growing knowledge of human genetics; secondly, wide acceptance of this knowledge as basic to social welfare; and thirdly, the connection of eugenic aims with those deeply instinctive drives to action which in all ages have led men and women to suppress individual desires in order to promote the well-being and survival of the group as a whole. In spite of present indifference ultimate success is not impossible. Certainly no other dream of the human future seems to promise so much; none other seems more essential to the improvement and even the preservation of the complex civilization now developing.

There seems to be far more agreement on the ultimate objectives of the movement, that is, to bring fertility into positive correlation with genetic worth, than on how to bring this about. It is now clearly seen that negative policies promise far too little. To get a positive program into operation will, as this journal has emphasized, take all the courage and staying power proponents can muster; and, I must add, also a favorable cultural conjuncture. Like some other commentators I'm not too hopeful, but I'm far from ready to surrender the dream as just another castle in Spain. One of the first steps surely is the clarification of the Eugenic Hypothesis. Like Robert Cook I feel that the various statements contain ambiguities and that some clarification of terms and basic assumptions is in order.

I wish first to discuss the wavering and unclear position taken on the basic issue of whether those who rise in the social scale are genetically superior to those who do not. It is stated that "there is some evidence" of better than average genetic capacities "among small professional groups" as compared with certain fringe groups at the bottom of the occupational scale. Nevertheless, there is repeated implication that recent differential fertility has been dysgenic and that the primary aim is to reverse it.

Part of this wavering seems due to the refinement of research methods showing a greater environmental influence in class differences in mental test results than was formerly suspected. That we all accept, though differences of opinion remain as to how much the total picture is altered thereby. Part of the wavering seems to me to be due to the change in the general climate of opinion since the days of Galton and Pearson. This point I wish to elaborate somewhat since it deals with the role of *Zeitgeist* as a factor in cultural change.

The increased emphasis on environmental forces in individual achievement is worldwide; it is associated with those basic changes in the socio-cultural situation which has led to a gradual substitution of the values of individualistic democracy for

the values and ideals of collectivist democracy, socialism and communism. Formerly "blood tells" was a popular maxim, the "self-made man" a popular hero and self-reliance and personal responsibility for success or failure deeply ingrained in popular morality. These frontier values, born out of generations of white-world expansion, were beginning to weaken around the turn of the century. "Rugged individualism" became an anachronism when the population, separated from the soil and local community, found itself largely in the grip of gargantuan industrial conditions over which it had no control but on the regular functioning of which livelihood depended. Success and failure alike came more and more to be attributed to good luck, family connection, favoring circumstance; society became responsible for poverty and criminality; mass man had to be insured against the contingencies of industrial-urban life or democratic societies might well have suffered revolution.

It is useless to question the validity of these new values, since in last analysis all active social values are pragmatic in operation. They must change with the social conditions or cease to have effect. Hence one must make up his mind that the culture drift will be a determining factor in the success of any eugenic program. If this is "the century of the common man," it may prove difficult to alter recent cultural trends. The average man has, like nearly all of us, a strong sense of family pride. He tends to reject claims of inherent superiority on the part of those above him in the social scale. I do not see, however, how the eugenic paradise can be attained on a voluntary basis unless there is in active operation a selective sense of family pride. That is, those who go in for large families in a eugenically conscious society must feel that they have more than average contributions to make to the genetic composition of the population while others must feel that they have less. There is already some indication that a eugenic conscience is operating negatively. Can it become a factor positively correlated with fertility?

Some such connection would seem to become more urgent as culture diversifies, opportunities broaden and talents of all kinds are more highly valued. It may be recalled that it was a cardinal principle of Jefferson and others of his persuasion that, in the absence of hereditary class privileges, universal education plus selection on the basis of proven merit would result in all types of social activity being led by "the natural aristocracy," that is, the inherently more able. Nor does it seem possible to conceive of a better test of genetic worth than ability to rise in the social scale under such conditions. No such scheme works to perfection, nor any other selective process.

However, there seems to be an intensification of the selection in recent trends. Educational opportunities have expanded in school, college and on the job and outside it. Vocational guidance is being aided by an active search for promising youth. Scholarships and other aids increase. The spread of urban dominance over rural life has increased the pull of latent talent into the main streams of modern life, while the extraordinary diversification of skills gives increased opportunity for the exploitation of an ever-widening variety of genetic potentials.

For these and related reasons our society tends to become more and more finely graded according to genetic capacities for response to environing cultural conditions. The gap between top and bottom widens while the areas between tend to be stripped of their best genetic combinations. Moreover, every effort to improve the environment so as to bring more latent talent into operation throws more weight on hereditary factors as selective determiners. Here it is worth while to recall the rule long ago stressed by Thorndike that the talented move up faster and for a longer time than the less gifted.

The above contains part of my reaction to the question raised by Robert Cook as

to whether those who rise in the social scale carry with them "a whole constellation of desirable traits" inherited *en bloc*. Under the social selective conditions of a competitive democratic society it would seem to take a better than average combination of traits to rise. It is not that these are inherited as a unit; that would violate the law of independent assortment. It is rather the nature of the collection of genes to be assorted, as is exemplified by a good many family strains that maintain a high degree of achievement generation after generation, even though they also produce some social failures. Very possibly some degree of homozygosity checks regression, a condition favored by the rather high degree of assortive mating.

May I add that, if the above picture of the social selection of genetic potentials is not more or less correct, then we as eugenicists are clearly on the wrong track. We should become social revolutionists seeking to put such selection to work in the interest of social dynamics. If the selective processes bring to positions of power and leadership a purely random selection of genetic potentials, then democracy has failed of its promise. However, it seems to be the basic assumption of the Eugenic Hypothesis that these processes have worked almost too well, so that differential fertility threatens future quality. As the demand for talent rises and the search for it intensifies, it would seem that there might well arise among the more thoughtful leaders in all walks of life an increasing realization of the importance of the genetic basis of the population for the continued advancement of culture and the maintenance of our world position.

We come to the question of the genetic association of love of children with those traits needed for success in cultural achievement. That it is high seems doubtful. Love of children is basically instinctive and, except as structured and pressured by the mores, not much stronger than needed to insure the survival of the species. At least customs of infanticide, abandonment, trading, selling and abuse of children are far from uncommon in human history. Even today every community must care for neglected and abandoned children; it must use pressure to compel some parents to feed, clothe and educate their children, often with poor success. If love of children is genetically tied to those traits needed for success in responding to the cultural environment, then such association must be proven, not merely assumed, or one must show how it can be bred into the race.

Then there is that ubiquitous word "responsible." I can see that it avoids the necessity of saying directly that "upper" classes should have larger families than "lower," (a fruitless concession to the *Zeitgeist*) but that seems to be the implication of most passages. In any case it seems to me that a sense of family and social responsibility may be somewhat associated with those traits needed for wide responses to the cultural medium. If it could be positively correlated with fertility, we would have our eugenic paradise. The trouble is that it is negatively correlated, hence all our worries. One may recall what Malthus had to say about "prudence." Responsible parents tend to be prudent, rational in mentality and imaginatively calculating in estimating the future. If they also participate widely in communal and other cultural activities, thus evincing a conscientious regard for wider social values, they are very likely to plan the size of their families with care and avoid serious miscalculations. The fact that within certain categories of the population, such as Yale graduates, the more successful have larger families than the less successful does not get us very far, since all such groups known to me had fertility rates below replacement levels. On the whole then, since love of children seems to have slight, if any, correlation with superior genetic endowment and the sense of responsibility leads to small families in our highly complex and

nerve-racking civilization, the manipulation of the cultural trend so as to stimulate the former in some and depress it in others and at the same time induce the responsible to act irresponsibly will take some doing. I would not say it cannot be done, but that comes close to the heart of the eugenic problem.

As to the question whether Lamarckism is implicit in the Eugenic Hypothesis, it has seemed to me also that some statements may have given that impression, though I feel certain none such was intended. The impression derives from the fact that euthenics is made the instrument of eugenics. It cannot be otherwise, since the size of family is so deeply affected by social standards. In the second place, the claim that families that attain high levels of culture will tend to produce children who do likewise seems to me merely a statement of what is well known. There are two kinds of inheritance, genetic and cultural or social. Parents transmit both kinds to their offspring. The children tend to inherit those capacities which enabled the parents to succeed; and the parents provide an environment in which these capacities can be developed. In fact, one of the beauties of the Eugenic Hypothesis is that, *if it could be put into operation,* it would set up a social scheme in which the long argument over the relative importance of heredity and environment in individual development would no longer be pertinent since both would work together. That, however, is a big "if" clause, even though some families today fulfill the eugenic ideal.

There is one further point in the Hypothesis that gives me pause. It is said that the aim is "to develop a community environment in which, by an unconscious and wholly voluntary process," more children will "be born in the homes most appropriate to child development," but that the size of family born to "responsible" parents will deserve neither approval nor disapproval by the community. This seems to me clearly visionary. It neglects the fact that size of family is largely a matter of style and thus affected by community approval. It neglects the fact that in nearly all we do we are governed by community attitudes of praise or blame. The planned family requires conscious effort, and responsible parents are certain to be quite aware of what they attempt and why. It seems to me that in a community with a well-developed eugenic consciousness pride of family will be strong among those with large families mainly because that will be one of the surest ways to win social approbation.

These remarks already too long should end on a more positive note. As I have said, success depends on favoring changes in the scheme of associated cultural values. The spread of birth control is reducing what is believed to be dysgenic birth differentials. This seems likely to continue. What else? It would seem that there will be an increased appreciation of the genetic factors in social success as the demand for talent of many kinds and degrees becomes more intense and the agencies for its cultivation and discovery broaden. Thus eugenic consciousness should spread.

Such spread should be favored by the promising advances in the science of human genetics. As practical applications of such knowledge are more widely discussed, eugenic consciousness should spread still more widely. Here one touches on a subject worthy of more study, namely, through what processes and agencies do scientific findings affect the habits and values of the general public? It would seem almost certain that the inherent rationality of western man has changed little for several generations, but his culture has become more rational as is shown by the increase in longevity. This view rejects the contention of Charles G. Darwin that mankind will become neither more rational nor more virtuous during the next million years. Perhaps not genetically, but his daily life and mores will certainly be affected by ad-

vancing knowledge and his moral and legal codes will change in deference thereto. Advances in human genetics should thus affect the mores of marriage and family size and their legal setting. Our so-called "eugenic laws" seem likely, therefore, to be broadened to include voluntary sterilization and therapeutic abortion under medical suprvision.

All the foregoing are only essential preliminaries to a truly positive eugenic environment. To bring fertility and eugenic worth into positive correlation requires two additional changes, both difficult but neither impossible. The first is the attachment of the eugenic ideal to that almost universal willingness of individuals to sacrifice some degree of personal pleasure for what they feel are ends larger than themselves, that is, those social values believed essential to the enduring welfare of their society as a whole. Here again we have an instinctive and impulsive emotion which may be attached to either constructive or destructive social purposes and which reacts powerfully to propaganda appeals. Eugenics, however, being based on scientific advances, seems likely to escape ideological excesses so long as it does not lose its humanistic character.

There remains still the problem of how to make the fertility rate positively selective in the genetic sense. Here I see no solution unless by some subtle change in culture associated with an intense eugenic consciousness it should become fashionable for those whose success shows superior capacities to respond to the cultural medium to have large families, while those of lower attainment should have smaller ones. Fashion is a powerful force since we all want to be well thought of, but this suggestion, like some others, sounds to me like autistic thinking.

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BOOK REVIEWS

THE HUMAN HEREDITY HANDBOOK: Amram Scheinfeld, J. B. Lippincott Co., Philadelphia, 1956, 276 pp. $3.95.

The public has always been interested in science and in the activities of scientists. Until relatively modern times it was possible for the layman to read the writings of scientists with understanding. This is no longer so. Today, not only is the non-scientist a layman, but so also is every scientist in all fields except his own. Yet, at present, the interest of the layman (and I now use the word in its broadest sense) in science and his need to know about science are greater than ever. Science, therefore, must be "interpreted" for him. Most scientists either cannot, or will not, undertake this task. Fortunately, some highly gifted non-scientists have undertaken to do so. The author of the present book is one of the most talented of these.

Amram Scheinfeld was one of the first to interpret human genetics into language which could be understood by the non-geneticist. He won richly deserved acclaim for his excellent book *You and Heredity* (1939) and for its revised edition entitled *The New You and Heredity* (1950). His intriguingly entitled book, *Women and Men*, (1944) was a highly successful description of the "combatants" in the war between the sexes in psychological, physiological, anthropological, and genetic terms.

In his earlier books Mr. Scheinfeld was translating technical material for the layman to enable him to ask intelligent questions and to know when to seek advice. In this book the author has undertaken a different task. He says, "The Handbook has been planned . . . to serve [as] a quick, handy, untechnical . . . guide to the general subject of human inheritance, and to provide direct, *uncomplicated* answers to such specific questions on the subject as may be asked by parents, . . . teachers, students, *medical men, marriage counselors,* and other interested persons," [Italics mine —A.G.S.].

This purpose implies to me that the book is meant to be used as a counseling guide; as a desk reference book for the physician to answer parents' questions; for marriage counselors to advise parents; for parents to guide themselves, and so on. The emphasis is on answers to specific questions.

In 30 chapters the author discusses 29 different topics, among these are a superb discussion of heredity and environment, and discussions of such items of popular interest as sex determination, twins, eye color and hair color, the features, sex and sexual behavior and so on. These largely parallel the discussions in the earlier books. The major new items concern the inheritance of organic diseases, and other defects and abnormalities, including mental diseases. The chapter on mental diseases is a good discussion of the problem, and has been reprinted without change in the May 1956 issue of the A.M.A. publication, *Today's Health.*

Unfortunately, Mr. Scheinfeld's discussions of other morbid characters do not maintain the standard of this chapter. In his attempt to be "uncomplicated" Mr. Scheinfeld has omitted *necessary* complications which limit generalizations. These complications cannot be ignored in evaluating specific genetic problems. In his attempt to supply "quick, handy, untechnical" answers, he has often been inaccurate or misleading. Some inaccuracies result from the presentation of complicated material in simplified form. There is no objection to such simplification provided the author clearly indicates what he is doing. Mr. Scheinfeld has not done so in this book.

Some specific illustrations will help to make my point. The discussion of genes and their action (page 7) which is presented without qualification has been in-

acceptable to geneticists for almost 20 years. The discussion of gene and chromosome duplication (page 16 and fig. on page 17) is presented as fact, yet it is not known how, or even when, this occurs. The tendency to label genes as good, bad, etc. without ever indicating that these are circumlocutions is carried to unfortunate extremes. A mutant gene whose presence leads to a malformed individual is not "a defective gene" any more than a bullet which maims an individual is a defective bullet. It seems to me to be necessary to explain to the lay reader that when a geneticist uses the expression "defective gene" he means a gene whose physiological effect on the organism which bears it is such as to render the organism defective in a given range of environments. This lengthy statement has been replaced by the short statement "defective gene." Mr. Scheinfeld has failed to indicate that this substitution is verbal shorthand used by the geneticist for convenience.

He has made statements which are misleading. Examples: page 88: The discussion of breast cancer is needlessly alarming. Present evidence indicates that first degree female relatives of women with breast cancer run only approximately twice as great a risk as the general population. The expected death rate for such women past 34 years of age is less than 2 per 1000. The discussion of intestinal cancer (page 90) is unfortunately worded. It could lead one to believe that a large proportion of intestinal cancer is due to a dominant gene.

He has included material which is still in the early stages of investigation and at least some of which may be wrong. The layman has no way of evaluating this. Such information can seriously confuse or mislead. He has attempted to explain too much, including "How Genius is Produced."

As in all new texts, there are several misprints. The most serious of these is in the figure describing sex-linked inheritance (page 79). An affected man's sons are said to have inherited a normal gene from the father. Clearly, the author meant to say mother.

In the glossary of "Terms not defined in this book, but which the reader may encounter in other books . . ." agglutination, alleles, genetic drift, Hardy-Weinberg law, and mitosis are inaccurately defined. In each case the reader would draw incorrect inferences from the definition.

I am sorry to have to be so negative about this book. It is good to explain genetics to people to enable them to ask intelligent questions and to know when to seek advice. Scheinfeld has done this very well in his earlier books. It is, however, misleading to publish oversimplifications without stating they are oversimplifications, to lead laymen and professional people (including medical men and marriage counselors for whom, among others, the author claims to have written this book) to think they have enough information to interpret genetic material. It is important to remember that genetic counseling deals with human emotions, human happiness, and often human lives. It cannot and must not be oversimplified and reduced to cookbook formulas.

This reviewer hopes that Mr. Scheinfeld will make the necessary changes which will enable the book to fulfill the purpose he has set for it.

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EDITOR'S NOTE:

Amram Scheinfeld, author of The Human Heredity Handbook, has written the following comments on Dr. Steinberg's review of his handbook.

To the Editor:

I am grateful to my friend Dr. Arthur Steinberg for affording me a "preview" of his report on my Handbook, and to the *Eugenics Quarterly* editors for inviting me to comment in detail.

This is the first time that I ever have taken public issue with a reviewer of one of my books. I do so now, and at such length, because many of Dr. Steinberg's criticisms go beyond my book to the whole problem of popularizing human genetics and extending its usefulness; and also because we have had some detailed correspondence which must be drawn upon to clarify the points at issue. Specifically, the review as it appears now is a revised draft. Dr. Steinberg was kind enough to send me a first draft for appraisal, and after I took exception to various statements and omissions of pertinent facts he conceded, "The review is too one-sided, and I have modified it accordingly." However, his redrafted review not only retains some of the statements I challenged, but interpolates some new points which I regard as compounding the previous errors.

To begin with, and as the springboard for his major criticisms, Dr. Steinberg contends that my Handbook must be judged more severely than my previous books because it was intended, *he deduces,* to be a "genetics counseling guide." This just isn't so. The original prospectus, prepared for my publishers (and sent to Dr. Steinberg), shows clearly that the book was intended to be only a *simpler,* more concise and less technical book than *The New* YOU AND HEREDITY, which had been upgraded and expanded from the original edition and adapted more for college and professional uses. Nowhere does the Handbook state that it is "meant to be used as a counseling guide," or "for parents to guide themselves." On the contrary, a list of CAUTIONS (p.84) specifically says "The non-medical reader should not try to draw precise conclusions about the hereditary threats of any condition without having had competent medical opinion beforehand," and "The doctor should study the family history—check medical literature, and/or consult with a medical geneticist."

On p. 243 I note that while "general answers" can be found in this book, more specific guidance should be sought from qualified persons. And I clearly limit and define the guidance functions of the physician and marriage counselor, and direct the reader to the medical geneticist in all situations which are chiefly genetic.

Despite all this, Dr. Steinberg prefers to endow my little book with ominous significance, and insists that it wasn't meant to be what my publishers and I meant it to be (and what a casual glance might show it to be), but what he himself has concluded it was meant to be—a "genetic counseling guide" or a "genetic cookbook"—and that it must be judged accordingly. (This is almost like saying that what was bred and reared as a pony is actually a horse, and must be flayed for not doing a horse's job.) In short, the very last words of the review might more correctly have been written—"the purpose the reviewer has set. . ."

Again, contrary to what Dr. Steinberg infers about my previous books (to justify his praise of them), they definitely were not written "to enable the layman to ask intelligent questions." People do not buy books about human heredity so they can be helped to ask questions; people are already chockfull of questions. They want answers, and the jackets of my previous books explicitly stated that hundreds of answers were being given. The present book most certainly also gives answers, but the big majority of these relate not to genetic counseling problems (the public interest in which I believe Dr. Steinberg greatly overrates), but to such everyday topics as prenatal impressions, sex determination, children's looks, twins, personality, intelligence, longevity, sexual behavior, ancestry, race differences, atomic-radiation effects, evolution, sterility, adoptions, disputed paternity tests, artificial insemination, etc. It is questions on such topics that I have found through long experience to be of much more popular interest than those regarding pathological and abnormal

traits with which, unfortunately, research in human genetics has been so disproportionately concerned. (I do not mean there should be less of this research, but only that there should be more research with respect to "normal" traits.)

The review also complains of my having "omitted necessary complications." What is here "necessary" is open to argument. Thus, I feel that the simplified "string-of-beads" and similar explanations for genes and chromosomes still convey what is necessary for the average reader's easiest comprehension, and that to dwell on the uncertain complexities of the gene theory would be unnecessarily confusing.

So also, I have not regarded it improper in my popular books (nor have some of our leading geneticists in their own writings), to describe genes as "bad, good," or "harmful," "abnormal," "defective," etc., although I *do* make it clear repeatedly, despite what Dr. Steinberg says, that a gene's behavior may be much influenced by its environment. His comparison of genes and bullets is inapt. The differential effects of two given genes may depend entirely on their inherent differences, which is not true of bullets. But a bullet, like a gene, can also be "defective" or "abnormal" in its construction and workings (which is why there is army inspection of ammunition), and if so, its accuracy or effects will not be the same as that of a "normal" bullet.

In his exceptions to my breast-cancer discussion, Dr. Steinberg cites an "approximately twice as great" as the general risk for women with afflicted first-degree relatives, although even the studies to which he refers in our correspondence (Penrose, *et al.*) show the risk as much closer to triple (61 observed, 23 expected). But he ignores other studies giving a greater risk, for instance, Jacobsen's, (cited in Neel & Schull, *Human Heredity*, 1954, p. 297), with a five to tenfold estimate. The best way of deciding whether I have been "needlessly alarming" is to read the full paragraph (p.88 of my Handbook) to which Dr. Steinberg refers:

> *Breast cancer.* Despite conflicting reports and theories, there is enough suspicion of hereditary influence in breast cancer so that if several women in a family have been or are afflicted by it, the daughters or sisters should have frequent periodic examinations in their mature years, and prompt treatment if there are any questionable symtoms. However, the appearance of breast cancer in several successive generations — grandmother, mother, daughter—need not of itself prove inheritance, first because the condition is so common that it might be merely coincidence; second, because the breast cancers in different members might be of different and unrelated types, which had started out at different locations.

(I might add that my discussions of breast cancer and intestinal cancer, as of other conditions, were prepared with the aid of and checked in manuscript by competent medical geneticists.)

An odd criticism is that I have "included material . . . still in the early stages of investigation," etc. I would hardly apologize for being up-to-date. And, happily, the old argument that the layman should be kept ignorant of tentative (even though reputable) reports in science for the reasons the review gives is finding less and less support in medical and scientific circles.*

With respect to my "Glossary"definitions, in the case of only two of the five called "inaccurate" has Dr. Steinberg told me what his objections are. Here is one of these definitions:

> "*Agglutination.* Clumping in the blood when there is a transfusion involving persons of conflicting blood types."

Dr. Steinberg's reason for calling it "inaccurate": The clumping may also occur in a test tube.

*The Bulletin of the Amer. Inst. of Biolog. Sciences, in its review of my Handbook (June, 1956, p.23), commends it for being, for its size, ". . . exceptionally broad in coverage and quite up-to-date on the recent discoveries and developments."

My "*Holandric inheritance*" definition happens to be almost exactly like that given by two outstanding geneticists in a recent textbook. Dr. Steinberg says we are all of us inaccurate because we speak of a "dominant" gene in the nonhomologous section of the Y chromosome. While such a gene unquestionably acts as a dominant, he says it must not be called a "dominant" because it has no matching allele, and is therefore neither heterozygous nor homozygous, but "hemizygous." A fine technical point, but whether it warrants elucidation in a popular book (with the odds being one in who-knows-what that any reader has an Holandric trait) is debatable.

I fully agree with the review's conclusion that "genetic counseling deals with human emotions . . . happiness . . . and often, lives." But since my Handbook, as I said before, is not offered as a genetic-counseling text or a genetic cookbook, I can only pass along Dr. Steinberg's accompanying admonitions to those who do write such books.

My most serious difference with Dr. Steinberg is in his reiterated contention that human genetics should be explained to people "to enable them to ask intelligent questions and to know when toseek advice." As he is fully aware, there are as yet only a handful of qualified persons who can provide answers such as he prescribes, and these are being and can be called on by only a tiny fraction of the public. If only for the present, therefore, the layman must obtain his answers about human genetics mainly from what he reads. Apart from this, I believe Dr. Steinberg greatly underestimates the intelligence of laymen (particularly since, in his opening paragraph, he includes among them even scientists, doctors and other professional men who are not geneticists) if he fears that providing them with simplified answers to genetic questions will have dire consequences. Rather, my own feeling is that the average person, if given the basic facts about human genetics, will draw sensible conclusions, whereas he might be confused and misled by scientifically precise, but complicated and qualified expositions of genetic facts (if he bothers to read them at all).

Finally, I hope it will be clear that this has been not a personal argument, but a professional one, dealing perhaps more with semantics than with genetics. Whatever my dissent from the review, I am certain that Dr. Steinberg—one of the most competent of our human geneticists—has written only in what he considers the best interests of the science; and to the degree that he has stimulated discussion and brought some moot and important points into the open, I feel sure that he has done so. For my own part the discussion has been a rewarding one, and may well lead to improvements in my Handbook if it is privileged to have further printings.

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New York City
June 21, 1956

MAN'S NATURE AND NATURE'S MAN: The Ecology of Human Communities, Lee R. Dice, University of Michigan Press, Ann Arbor, Michigan, 1955, 329 pp., $5.00.

"The major thesis of this book is that man is able to exist only as a member of some community in which associated species of plants and animals supply him with food and other esential supplies and services." Thus does Professor Dice begin the sequel to his earlier volume, *Natural Communities*, this time viewing the human community within the ecological frame of reference. His definition of community is broad, having little relationship to the sociologist's usage. For Dice, "we speak of a human community when our attention is directed chiefly to man and to his relationships with the various species associated with him in a particular situation."

In simple, lucid sentences, the exposition

moves from the essential conditions for existence of the human organism, through brief characterizations of man as a social animal, the dynamics of human populations and the evolution of human heredity, to the three-fourths of the volume devoted to aspects of community. There is a constant interweaving of data on natural communities with references to man. Very often the human illustrations relate to primitive man, whose relationship to his natural environment is more easily delineated than is that of civilized man. Toward the end of the volume, Dice presents his own "philosophy of the human community." The human community is seen as "the highest level of organic evolution that has yet appeared on the earth." Moral codes, as well as technology and human customs generally, are seen as evolving "toward a more or less harmonious relationship with the conditions for human life in the area concerned."

Professor Dice draws upon diverse fields of knowledge to present man in his natural setting. His style of writing is exquisitely simple. He reminds us of the things that are obvious, as well as of things which we may not have known. Some chapters, especially the first three, consist in large part of assertions that would be accepted as obvious by most junior high school students. Indeed, if one considers again Dice's thesis, it requires very few simple assertions to document the fact that man is dependent on plants and animals for "food and other esential supplies and services."

Nature's man is an organism, dependent for existence upon other organisms, human and non-human. But man's nature and man's life in human communities is not to be adequately understood in terms of this dependence. In the chapters where Dice discusses man as a social animal (whose two fundamental drives are egocentric and altruistic), the human society and moral codes, he writes with a minimal use of the findings of any of the behavioral sciences except anthropology during the past two decades. He neither mentions, nor lists in his bibliography, such classical studies of the rise of urban communities and the relationships between the ecological order and the moral order as the works of Pirenne, Gras, McKenzie and Park. A wealth of research by demographers, urban sociologists, economists and historians has been devoted to the social ecology of human communities. It is to be regretted that Professor Dice did not include more of the findings of this research in his synthesis. On the whole, this volume seems to this reviewer to have elaborated a thesis less significant than the volume's title would demand.

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POPULATION: Dennis H. Wrong. Random House Studies in Sociology. Random House, New York, 1956. 128 pp. 95c.

This pocketbook gives a clear and workmanlike summary of the present state of knowledge in the core areas of demography. The chapters covering the delimitation of the field, the history and prospects of world population growth, and its component factors of mortality, fertility, and migration might serve as lectures in an introductory course. Another chapter, based on the author's doctoral dissertation on "Class Fertility Trends in Western Nations," gives more attention to differential fertility, and a concluding chapter to the Malthusian and Neo-Malthusian controversies and national problems of food procurement. The author explains that for lack of space he has to omit such important subdivisions as "the statistics of marriage and divorce, population forecasts, measures of the intergenerational replacement of population, governmental population policies, and the relation between population and national power." He presents enough statistical illustration to make typical orders of magnitude and significant relationships familiar.

His point of view is that of a serious sociologist, thoroughly imbued with the institutions and values of Western culture. One may question at times his reliance on analogies between Western experience and the problems of underdeveloped countries, and at times may feel that he underestimates the urgency and scale of some of these latter. One could also wish that since most of his data are given for national or similar administrative units, he had found space for some mention of nationalism and national policy as factors in demographic development.

It is in the chapter on differential fertility that he briefly discusses eugenics. Citing data from the United States, Great Britain, France, Germany, Scandinavia, Canada, and Australia, he has discussed the timing and patterns of variations in fertility performance between intranational groups based on religion, race, national origin, and especially those based on social classes, defined by occupation, educational attainment, income, ecological or other factors. In conclusion he takes issue with the theory that the differential fertility of socioeconomic classes, in which high IQs are positively associated with high status and fertility is negatively associated, leads to the erosion of the genes controlling intelligence in the population. He feels that the evidence for a final decision on the theory is not yet all in, but he points to the possibility that the whole debate may become irrelevant if the present trend toward equalization of class birth rates continues.

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EXPLORING THE MUSICAL MIND: Jacob Kwalwasser, Coleman-Ross Co., New York, 1955, 189 pp.

This book examines the relationship between musical talent and a group of variables including inheritance, race and nationality, intelligence, age, sex and training, devoting a separate chapter to each factor. Additional chapters consider measurements of talent, tongue agility and other motor skills, and music reading. The author's avowed purposes are to disseminate the results of organized experimentation in musical talent and to interpret the results of such investigations in terms of more effective pedagogy. As co-author of a widely used musical aptitude test, the Kwalwasser-Dykema, and as Professor Emeritus of Music at Syracuse University, the author has many qualifications for this task. Combining data from the literature with the results of his own investigations, the author presents statistics which he believes support two assumptions, that heredity plays the major role in determining musical aptitudes, and that teaching which ignores innate differences is likely to be unproductive. Both assumptions may be correct, but the evidence which he presents in support of the former will be regarded as neither critical nor sufficient by most students of heredity. The author recognizes some weaknesses in the available evidence and the need for further research.

The first chapter, musical talent and inheritance, is disappointing, in part, because of the dearth of studies in this area. The author draws heavily upon material of limited value from Scheinfeld's *You and Heredity*. The presence of obvious errors is a greater source of disappointment. One reads this laudatory citation of another writer's mistaken notion, "With convincing logic he (O'Connor) shows that a single completely isolated recessive trait, in the simplest Mendelian sense, occurs theoretically once in four persons." It is apparent that both authors have erroneously equated the probability of segregating (from a mating of two heterozygous parents) an individual with the homozygous recessive genotype with the frequency of homozygous recessive individuals in the general population. Based on this misunderstanding, subsequent calculations presented on the seg-

regation probabilities of traits governed by many gene pairs are meaningless. Later the author writes, "The pedigree chart is more easily comprehended. It is based on a more superficial treatment of data. . . ." It cannot be denied that *some* studies have unfortunately been based on a more superficial treatment of data, but the author's statement ignores the fact that, as records of the occurrence of traits among related organisms, pedigrees form the base for all genetic investigations.

The author cites three studies providing suggestive evidence of heredity's role in musical aptitudes. All employ the Kwalwasser-Dykema tests which partition aptitude into components such as pitch discrimination, tonal memory, rhythm, etc. In the studies presented, each sib's scores on all components are summed, the total score being used for sib comparisons. One study yielded a correlation between 71 brother-pairs of +.54, and between 65 sister-pairs, +.46. A twin study (25 pairs, zygosity unspecified) produced a correlation of +.77. The third investigation reported a correlation of +.53 between siblings in 151 families.

The book includes several carelessly worded or dogmatic statements which are largely unsubstantiated by the data presented such as the following from the chapter on race and nationality: "While the Negro scores relatively higher in music talent than he does in intelligence tests, he is inferior in both, when we take the trouble to compare earned averages of large school populations." One objection to this statement is the author's implication that these differences reflect hereditary differences while the assumption that heredity has anything to do with musical aptitudes remains untested. In addition, the fact that the average test scores of American children grouped according to national origins differ significantly in all cases reported from the average scores of children now living in the corresponding countries suggests that strong environmental influences are affecting the expression of these traits. Moreover, the differences observed between mean scores of American national or racial groups are only about half as great as those observed between American groups and their European cognates. Finally, I see little pedagogical value in such comparisons because the intra-group variability greatly exceeds inter-group differences.

Later chapters in the book contain fewer inaccurate and incautious generalizations and many interesting observations. The slight influence of age, sex, and training on aptitude test scores (in a "uniform" environment) doubtless strengthened the author's faith in the hereditability of these traits. Possibly careful pedigree studies employing single components of Seashore's or the author's tests may reveal major genic differences. A study of families attempted by Kwalwasser failed because parents were test-shy. Certainly, the need for knowledge of additional common allelomorphic differences should encourage investigations of this kind.

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STANDING ROOM ONLY: The Challenge of Over-Population. Karl Sax, Beacon Press, Massachusetts, 1955, 206 pp., 13 figures, $3.00.

Had this book been published ten, or even five, years ago, it would have been possible to recommend it warmly. It is still a readable introduction to the subject of overpopulation and might well be given to any person beginning to become acquainted with the field; most of it would be familiar to anyone reasonably well-read in the subject.

The name of the late J. C. Flugel is strangely missing from the bibliography despite the fact that his writings have done so much to throw light on the behavioral road-blocks to a rational approach to control of human fertility. On the other hand,

Dr. Sax reduces Josué de Castro to a cinder.

The book, written with courage and candor, documents more completely than any other work I can remember the recent history of Roman Catholic opposition to birth control, in word and deed. In this democracy of ours, some of Dr. Sax's citations should be pondered well. His table of birth rates in Massachusetts cities that are predominantly Roman Catholic is significant. As the people are said "to be ahead" of the Congress, they seem also to be ahead of the clergy. The latter, in some cases, seem not to have caught up with the Church.

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PERIODICAL ABSTRACTS

GENETICS

Prospects of biochemical genetics in medicine. J. A. Böök and R. Kostman. *Annals of Human Genetics* **20** (1956), pp. 251-252.

Many difficulties now confronting human biochemical genetics may eventually be overcome by tissue culture methods. In the case of one disease, infantile genetic agranulocytosis, cell cultures have already yielded suggestive information. In this disease, granular white cells are nearly absent from the blood, and death from infection occurs in infancy. It was found that patient's bone marrow cultured in his own serum showed complete inhibition of white cell formation and partial inhibition of red cell formation. Addition of the amino acid, cysteine, to these cultures partly overcame the inhibition, as did cultivation of the same marrow in serum of normal individuals.

Natural selection associated with the ABO blood group. J. A. Buckwalter, *et al., Science* **123** 1956), pp. 840-841.

Differential mortality among persons of different blood groups is an expression of natural selection operating to change or to maintain the composition of the population. A British study, reported in 1953, revealed a higher than normal incidence of group A among patients with stomach cancer, and a higher than normal incidence of group O among patients with peptic ulcer. A large study has now been carried out on Americans with confirmatory results. In a control population, without cancer or ulcers, 46 per cent were group O and 42 per cent group A. In patients with stomach cancer the figures were 42 and 46, respectively, and in patients with peptic ulcer, 53 and 37. These differences are statistically significant.

The ABO blood groups and the sex ratio. B. H. Cohen and B. Glass. *Human Biology* **28** (1956), pp. 20-42.

Among babies of blood group A born to group A mothers the proportion of males is low, and among group B babies born to group B mothers it is high. This was found in two series of Baltimore cases totalling 4538 mother-offspring combinations. The authors suggest a genetic explanation of the deviant sex ratios and outline a means of testing their hypothesis.

A case of uniovular twins of which one child was normal and the other had the syndrome of mongolism. J. A. van Beukering and J. D. Vervoorn. *Acta Geneticae Medicae et Gemellologiae* **5** (1956), pp. 113-114.

A pair of Negro twins born at a hospital in Gold Coast, Africa, had a common placenta but two separate amniotic sacs. One baby was mongoloid, the other normal. Microscopic study of the membrane which separated the twins revealed two layers of amnion but no chorion. On this basis the twins were regarded as monozygotic. Blood group and fingerprint comparisons could not be made.

While histological examination of the membrane would generally be regarded as decisive, it is probably not infallible. If additional evidence were available, this case would constitute a very important exception to the general rule about mongolism in twins, namely, that monozygotic twins are always concordant (similar) with respect to the defect.

Hereditary deaf mutism, with particular reference to Northern Ireland. A. C. Stevenson and E. A. Cheeseman. *Annals of Human Genetics* **20** (1956). pp. 177-231.

The hypothesis which best accounts for hereditary deafness seems to be multiple independent recessive factors. In a careful survey of Northern Ireland 424 persons were found who were thought to be born deaf, a prevalence of .031 per cent. After allowance for possible cases of nonhereditary deafness, the prevalence becomes .023 per cent. The authors' study of their patients confirmed the general finding that hereditary deafness represents diverse anatomical defects, but that discrete types could not be separated clinically.

Genetic, as well as anatomical considerations support the assumption of multiple recessive

types. If hereditary deafness were all due to one recessive gene, deaf-by-deaf matings should produce only deaf offspring. Actually, about five-sixths of such matings produced some or all hearing children, indicating that the marriage partners owed their deafness to independent genetic factors. The high frequency of cousin marriages among the parents of deaf is also more compatible with many independent genes than with one or two.

Fertility of deaf persons was estimated, on the basis of males in this study, to be only about one-third of normal. Since deafness appears to remain at a constant frequency in the population, the authors conclude that the deafness genes are replenished by new mutations or by greater fertility in heterozygous carriers.

Legendary Thai tribe. (Based on report by R. W. Weaver.) *Science* **120** (1956), pp. 880-881.

While investigating minor ethnic groups in the interior of Thailand, an expedition of the American Museum of Natural History discovered eight lone survivors of a nomadic group known in legend as the "Spirits of the Yellow Leaf." There remained six men, one woman and a small boy. All, and especially the woman, were diseased, and their lives were constantly threatened by tigers. These people had the most primitive culture imaginable. They did not engage in social conversation or competitive sport, and the child had no toys. The expedition treated the woman with modern drugs, but her chances of survival appeared poor.

Seldom before have students of human evolution seen such an impressive example of "drift," be it genetic or cultural. These people must once have been relatively numerous and healthy, with a culture close to that of more successful related tribes. When they tried to adapt to a too-hostile environment, their numbers diminished and they presumably lost valuable genes and even some basic elements of a once adequate and unique culture.

Other instances of "drift," resembling what occurred early in the history of the above tribe, were probably responsible for many of the important advances in man's early biological and cultural evolution. Since it is almost a random process, however, it must have led the great majority of deviant groups into blind alleys, to the forlorn ending exemplified by these Thailanders.

Population

A dynamic approach to the study of population growth with special reference to India. J. N. Sinha. *The Indian Economic Journal*, **3**, 2 (October, 1955), 177-191.

The chief purpose of this article is to emphasize the necessity of studying population change as part of a complex socio-economic system rather than as an essentially independent process. The author notes the widespread failure of population predictions to furnish an accurate guide to the future and stresses the need for empirical research as a complement to deductive reasoning in any attempt to formulate assumptions as to trends in mortality and fertility. A consideration of the population projections for India given in the All-India Census Report of 1951 leads to the conclusion that demographic developments should be viewed in terms of differentials among social classes and groups. Data furnished by an urban sample of 1000 families are offered in support of the hypothesis that fertility in India varies with income and with education. Figures from the same study are used to demonstrate a computation designed to obtain an indication of the trend of fertility from information gathered in a single survey. In the author's opinion, the family planning program of the Indian Planning Commission errs in giving relatively low priority to studies of motivation. "Research on the social and cultural determinants of attitudes towards family planning must take precedence over that on techniques [of planning]" (p. 189).

Social and demographic factors in metropolitan planning—with special reference to New England. Ralph Spielman. *Journal of the American Institute of Planners*, **33**, 1 (Winter, 1956), 2-10.

Taking the form of a summary of a situation rather than an exposition of new findings, this article begins with a discussion of the changing fertility behavior of the United States and comes to the conclusion that the baby boom is to be explained as a result more of the timing of births than of any return to a large-family pattern. The second principal topic treated is the phenomenon of urban decentralization. The way in which cities grow will be of such complexity and importance, it is asserted, that planning in terms of the territorial and political units inherited from the past will be increasingly difficult. Moreover, the relocation of the middle and upper classes "will doubtlessly contribute to the increasing crystallization of the class structure of the society" (p. 10).

Notes and comments on the meaning of residential propinquity as a factor in mate selection. Alan C. Kerckhoff. *Social Forces,* **34,** 3 (March, 1956), 207-213.

This paper begins with a report of a study of marriage license and census tract data for couples applying for marriage licenses in Davidson County (Nashville), Tennessee, in 1950. The most notable finding was the substantially greater propinquity of Negro couples as compared with white. Income, population density, and level of education were each found to be significantly, though very slightly, correlated with propinquity.

Proceeding to a discussion of propinquity studies in general, the author uses materials from Nashville to point up the difficulty of achieving comparability from one study to another. The importance of the geographical area included in the study, the choice of couples to be investigated, and the racial groups included is stressed.

The article concludes with a list of suggested areas for future research on the causes of variation in the propinquitousness of mates. Among these are such aspects of the "social context" as segregation, neighborhood organization, and neighborhood self-sufficiency; and such "individual variables" as breadth of intimate contacts, peer group rapport, mobility aspirations, and parental supervision. It is pointed out that the commonly used methods of propinquity research will have to be supplemented by on-the-spot observation if such subjects are to be investigated.

Les attitudes et la conjoncture démographique: natalité, structure familiale et limites de la vie active. Alain Girard and Louis Henry. *Population,* **11,** 1 (Jan.-Mar., 1956), 105-141.

During November and December of 1955, interviewers for the *Institut national d'études démographiques* questioned 2,553 persons in all parts of France with respect to their attitudes regarding family size, family formation, and certain other subjects. The sample was designed to be representative of the general population as to age, sex, rural-urban residence, and occupation. Marital status and number of children were not controlled, but the sample turned out to resemble the population quite closely in these respects.

The mean response to the question "In your opinion, what is the ideal number of children for a family?" was 2.85 for the entire sample, 2.84 for men and 2.86 for women. Almost all the responses (90.5%) lay between 2 and 4, inclusive. The actual family sizes of married respondents old enough to be well along toward completion of their families were much more widely dispersed than the reported ideal sizes. There was little difference in the mean ideal family size between the never-married, on the one hand (2.88), and the combined group of presently-married and widowed, on the other (2.86). The divorced reported a somewhat smaller ideal (2.33). For completed families, a systematic relationship appeared between ideal size and actual size, the ideal ranging from 2.62 for parents of one child to 3.94 for persons with six or more children. A similar correlation was found between the number of children in the respondent's family of origin and the respondent's judgment of ideal family size.

A second question, asking the ideal family size for persons in the same situation as the respondent, brought interesting results. About 75% gave the same answer as to the question about ideal family size in general. One fifth gave a lower figure and 5% a higher figure, so that the average became 2.63. There was a positive relationship between the economic status of the respondents and the degree to which their ideal for persons "in the same situation" approached their ideal for persons in general. The highest economic group had an average ideal for themselves (3.11) that even surpassed their ideal for families in general (3.06).

Other results of the study included these: 22% of the respondents hoped that the French population would grow; 57% hoped that it would remain stationary; 6% hoped it would decline. Fear of unemployment was the chief reason given by the latter group. The mean ideal age at marriage for men was 25.4 and for women 21.5. About one-third of the respondents thought it preferable that the first birth come "immediately" after marriage; two-thirds favored its coming "after a little while." Three-quarters of the sample preferred that later births come "with a certain spacing" rather than "rapidly." The article also sketches some of the information obtained from questions about labor force participation.

The authors conclude that though the building of families depends on more than the psychological attitudes of the individuals concerned, a knowledge of the collective ideals and their variations is a valuable guide to analysis. In the ideals discovered in this study they find no obstruction to the maintenance of the present level of fertility in France.

A critique of the Glass-Grebenik model for indirectly estimating desired family size. Robert G. Potter, Jr. *Population Studies,* **9,** 3 (March, 1956), 251-270.

This paper discusses at some length a model for estimating desired family size presented by

Glass and Grebenik in Volume VI, Part 1, (pp. 270 ff.) of the *Papers of the Royal Commission on Population*. After distinguishing among three different family-size ideals (size of family believed desirable for couples general, size of family believed desirable for couples in one's own circumstances, and size of family believed desirable for oneself), Potter offers evidence in support of the view that responses to questions about such ideals are not likely to give a reliable indication of the actual "intended" family size of a respondent, *i.e.*, the size of family he would have in the absence of sterility and with perfect birth control.

There follows an empirical and theoretical discussion of the extent to which the Glass-Grebenik model succeeds in furnishing a valid estimate of intended family size. Particular emphasis is placed upon the model's assumption that the risk of successive unintentional births is constant for any given couple. Certain computations are introduced to demonstrate that the risk of an additional unintentional pregnancy declines as the number of unintentional pregnancies experienced rises. In the author's view important improvement in the model would be achieved by taking this decline into account. "Provided that certain oversimplifications can be removed," Potter feels that "at the moment, the ingenious model of Glass and Grebenik seems to be the best bet to advance knowledge" in the area of intended fertility (p. 267).

A study of the growth of American families. P. K. Whelpton and Ronald Freedman. *American Journal of Sociology*, **61**, 6 (May, 1956), 595-601.

This preliminary report of an important study of the growth of American white families is devoted chiefly to an outline of the purposes that the study hopes to achieve and to comments upon these purposes. The six stated goals are as follows: "1. To obtain data on expectations about family size from young married couples as a basis for projecting births for the United States. 2. To improve our knowledge of the extent to which the postwar baby boom has resulted from an increase in the size of completed families and a change in the timing of children. 3. To describe for the nation the differences in fertility behavior between major population strata, defined by various socioeconomic and cultural data. 4. To explore the relationships between a limited number of 'basic' social and psychological variables, on the one hand, and fertility behavior, on the other." (Among the variables are: wife's activities outside the home, economic history and prospects of the family, wife's judgment of her "personal competence," social mobility, and respondent's perception of the completed family size of his associates.) "5. To examine the replies to open questions and probes bearing on family-size values and expectations (which were used fairly extensively) in a search for other variables affecting family size. 6. To formulate suitable methods of study." (Pp. 595 ff., *passim*.)

Interviews were conducted with 2,713 married women aged 18 to 39 and 250 single women aged 18 to 24, found by visiting 8,500 households selected by an area sampling plan. A notable early result of the study was its discovery of the excellent cooperation given by the respondents, which indicates that national data on family planning can be investigated satisfactorily by sample survey procedures. Only 12 of the 2,713 wives interviewed refused to answer questions about their family planning practices.

Publication of further results of the study will follow in due course. The reviewer, and others working in the field of population studies, will await with great interest the outcome of the attempt to use interview data as a basis for projecting fertility.

Social structure and fertility: an analytic framework. Kingsley Davis and Judith Blake. *Economic Development and Cultural Change*, **4**, 3 (April, 1956), 211-235.

This paper "represents an attempt to set forth and utilize an analytical framework for the comparative sociology of fertility" (p. 211). The framework consists of a list of eleven "intermediate variables" through which the influence of social factors may be transmitted to fertility. The list is as follows (p. 212): I. *Factors Affecting Exposure to Intercourse ("Intercourse Variables")*. A. Those governing the formation and dissolution of unions in the reproductive period. *1*. Age of entry into sexual unions. *2*. Permanent celibacy: proportion of women never entering sexual unions. *3*. Amount of reproductive period spent after or between unions. (a) When unions are broken by divorce, separation, or desertion. (b) When unions are broken by death of husband. B. Those governing the exposure to intercourse within unions. *4*. Voluntary abstinence. *5*. Involuntary abstinence (from impotence, illness, unavoidable but temporary separations). *6*. Coital frequency (excluding periods of abstinence). II. *Factors Affecting Exposure to Conception ("Conception Variables")*. *7*. Fecundity or infecundity, as affected by involuntary causes. *8*. Use or non-use of contraception. (a) By mechanical and chemical means. (b) By other means. *9*. Fecundity or infecundity, as affected by voluntary causes (sterilization, subincision, medical treatment, etc.) III. *Factors Affecting Gestation*

and Succesful Parturition ("*Gestation Variables*"). *10.* Foetal mortality from involuntary causes. *11.* Foetal mortality from voluntary causes.

In terms of the way in which fertility is affected through these variables in preindustrial societies, a reclassification of the eleven numbered factors is suggested (p. 214): Influence usually favors high fertility, Nos. 1,2,8,9; influence may favor high or low fertility, Nos. 3,11; influence usually favors low fertility; Nos. 4,10; influence indeterminate, Nos. 5,6,7.

The bulk of the paper is devoted to a preliminary analysis of the way in which different institutional patterns affect the several variables. Particular attention is given to factors 1,2, and 3, those governing the formation and dissolution of unions in the reproductive period; and least attention to 5,6,7, and 10, for which the comparative fertility-values (*i.e.*, direction, and degree of influence on fertility) are "patently unknown" (p. 234). The authors conclude that "it is possible to discern a systematic difference between underdeveloped and developed societies with reference to the eleven variables" (p. 234). The influence on fertility through the variables farthest removed from the moment of parturition appears to be relatively favorable (to high fertility) in preindustrial societies and relatively unfavorable in industrial societies. Preindustrial societies tend to postpone the control of pregnancy, whereas industrial societies tend to select whatever methods of control involve the least institutional organization and reorganization and the least human cost.

Readers of this article will perhaps find it of a somewhat awkward length, getting just far enough into some parts of the discussion to make one wish for a great deal more. Its stress on the need for orderly analysis of the effects of social factors on fertility is well placed, however.

George F. Mair
Smith College

NEWS NOTES

Schizophrenia

Dr. Franz J. Kallmann, head of the Department of Medical Genetics at the New York State Psychiatric Institute (Columbia University) and a director of the American Eugenics Society, has received a grant for a twin and sibship study of pre-adolescent forms of schizophrenia from the National Association for Mental Health. This was one of 22 grants for research projects on schizophrenia from a research fund provided by the Supreme Council, 33rd Degree, Scottish Rite Freemasonry, Northern Masonic Jurisdiction, which is directing the research program through the National Association for Mental Health.

Family Aid to Married Couples

Seven out of every ten married couples in the Detroit area exchange some kind of help with relatives outside their immediate family. Babysitting and care during illness are two of the most frequent ways in which relatives exchange help. One-half the families in greater Detroit receive or give these types of help. Financial aid and help with the housework rank next. Children are somewhat less likely to help their parents than to receive help from parents. And differences in family income don't seem to be associated with the frequency with which relatives help. These findings are from "A Social Profile of Detroit: 1955," a report of the Detroit Area Study of the University of Michigan.

Breast Cancer

The susceptibility of certain strains of female mice to breast tumors has been changed by genetically controlling the transmission of the mammary tumor agent. This is reported in a ten-year study of mammary tumors in more than 4,000 female mice of various genotypes, by Dr. Walter E. Heston, Margaret K. Deringer, and Thelma B. Dunn of the National Cancer Institute in Bethesda, Maryland.

This agent or virus disclosed a maternal influence in mouse breast cancer. When the young of a high-cancer strain mouse were foster-nursed by a female of a low-cancer strain, the young failed to develop cancer at the expected age. The experiment was then reversed, and many mice later developed breast cancer. These studies indicated that some factor in the milk was inciting the cancer and this factor seemed to have many characteristics of a virus.

In the search for gene or genes responsible for the transmission of this virus, a susceptible strain of mice was crossed with a strain which was resistant to the agent. As early as the third generation, the agent was completely eliminated by the concentration of chromatin (the carrier of the genes of inheritance) derived from the resistant strain. It is suggested that "these strains differ by only a few genes controlling the propagation and transmission of the agent and possibly by only a single pair of genes."

Heredity and Family Life

A meeting on Heredity and Family Life was held by the Oregon Coordinating Council on Social Hygiene and Family Life in Portland in May 1956. Dr. George W. Beadle, President of the American Association for the Advancement of Science and Head of the Biology Department at the California University of Technology, spoke on "New Knowledge of Heredity." Dr. Ralph Huestis, Head of the Biology Department at the University of Oregon, discussed "The Social and Psychological Effects of Different Environments on Identical Twins Reared Apart."

Public Health Service

Dr. Gordon Allen, a director of the American Eugenics Society, has been on active status with the Public Health Service for the past five years. Four years ago he was assigned to the National Institute of Mental Health and joined the staff of the Laboratory of Socio-environmental Studies which is headed by Dr. John A. Clausen. At Dr. Allen's request he has spent these four years at the New York State Psychiatric Institute studying mentally subnormal twins under the guidance of Dr. Franz J. Kallmann. In July of this year he returned to Bethesda, where he will continue his studies on mental deficiency and medical genetics.

FIRST INTERNATIONAL CONGRESS OF HUMAN GENETICS

The following program was presented at the First International Congress of Human Genetics in Copenhagen, August 1-6, 1956.

WEDNESDAY, AUGUST 1

OPENING SESSION

Address of Welcome by:
Julius Bomholt, M.P., Minister of Education.
Tage Kemp, President of the Danish Organizing Committee.

PLENARY SESSION

Mutation in Man.
Chairman: G. Montalenti.
Muller, H. J.: Further Studies Bearing on the Load of Mutations in Man.
Penrose, L. S.: Mutation in Man.
Radiation Genetics and its Human Implications.
Chairman: Sterling Emerson.
Neel, J. V., and W. J. Schull: Studies on the Potential Genetic Effects of the Atomic Bombs.
Carter, T. C.: Radiation Genetics and Human Populations.
Turpin, R. et J. Lejeune: Etude de la descendance de sujets traités par radiothérapie pelvienne.

SECTIONAL MEETINGS

A: INTERNAL MEDICINE

A1: Endocrinology—Cardiovascular System.
Chairman: A. Codounis.
Endocrinology.
Pyke, D. A.: The Incidence of Diabetes Mellitus and its Preponderance among Women.
Lamy, M., and J. Frézal: Hereditary Factors in Diabetes Mellitus.
Grunnet, J.: Heredity in Diabetes Mellitus.
White, P., J. Ditzel, and L. Sargeant: A Follow-up Study of Children of Young Diabetic Mothers.
Lamy, M., and J. Frézal: Heredity of Hemochromatosis.
Bernheim, M., M. Berger, R. Uzan et J. Chamberon: Le rôle des facteurs génétiques dans le développement du myxoédème infantile athyréotique.
Mosbech, J., E. D. Bartels, and H. Uhrbrand: The Protein-bound Iodine in Relatives of Patients with Thyroid Diseases.
Carter, C. O.: The Carrier State in Sex-linked Nephrogenic Diabetes Insipidus.
Cardiovascular System.
Miall, W. E., and P. D. Oldham: The Inheritance of Arterial Blood Pressure.
Roberts, J. A. Fraser: The Genetics of Essential Hypertension.
Kherumian, R. et J. Moullec: Le problème de l'étiologie génétique des cardiopathies congénitales.
Ayer, A. A., and G. H. Rao: Atrial Septal Defect in a Human Foetus Resulting from Deep X-ray Irradiation Administered to Mother during Early Pregnancy.
A2: Osteology—Congenital Defects.
Chairman: A. Polman.
Maltarello, A. A.: Aspects génotypiques du développement osseux chez les jumeaux MZ et DZ.
Degenhardt, K.-H.: Boebachtungen zur Phasenspezifität O_2-Mangel induzierter Wirbelsäulenmissbildungen bei Kaninchen.
Hnevkovsky, O.: Some Embryopathogenetic Factors in the Etiology and Prognosis of Dysplasia Coxae Congenita.
Hnevkovsky, O.: Analysis of 100 Pedigrees with Dysplasia Coxae Congenita.
DeMarinis, F., and A. Sobbota: On the Inheritance and Development of Preaxial and Postaxial Types of Polydactyly.
Stecher, R. M.: The Physical Characteristics and Heredity of Short Thumbs.
Witkop-Oostenrijk, G. A.: Contribution to the Study of the Inheritance of Dysostosis Cleidocranialis.
Fraser, F. C.: Etiologic Factors in the Clefts of the Palate and Lip.
McKusick, V. A.: The Genetic Behaviour of Heritable Disorders of Connective Tissue.

B: PSYCHIATRY—MENTAL DEFICIENCY—PSYCHOLOGY

Chairman: T. Sjögren and A. Lewis.
Allen, G., and F. J. Kallmann: Mongolism in Twin Sibships.
Reed, S. C., and E. W. Reed: The Relatives of Mentally Retarded.
Larson, C. A.: Some Aspects of Kin Matings with Mentally Defective Offspring.
Bosch, J. van den: Microcephaly in the Netherlands.
Marthinsen, R.: On Familial Mental Deficiency.
Kaij, L.: Drinking Habits in Twins.
Martensen-Larsen, O.: The Family Constellation and Alcoholism.
Martensen-Larsen, O.: The Family Constellation and Homosexualism.

C: POPULATION GENETICS — ANTHROPOLOGY —BLOOD GROUPS

Chairmen: R. A. Fisher and C. Gini.
Fisher, R. A.: Introduction: Blood-groups and Population Genetics.
Mourant, A. E.: Anthropology and Natural Selection of Blood Groups.
Roberts, J. A. Fraser: Associations between Blood Groups and Disease.
Aginsky, B. W.: Cultural Genetics.
Aschner, B. M., and R. H. Post: Modern Therapy and Hereditary Diseases. Estimates of Possible Changes in Gene Frequencies.
Cavalli-Sforza, L.L.: Some Notes on the Breeding Patterns of Human Populations.
Cook, R.: Change Pattern in Selection.
Slatis, H. M., and R. H. Reis: Understanding the Genetic Constitution of Man through the Study of Consanguineous Marriages.
Böök, J. A.: Genetical Morbidity of Children from First Cousin Marriages.
Rüdin, E.: Preliminary Report on the Offspring of Closely Consanguineous Parents.
Haldane, J. B. S.: Natural Selection in Man.
Osborn, F.: Changing Demographic Trends of Interest to Population Genetics.

D: NEUROLOGY

Chairman: S. Refsum.
Becker, P. E.: Neue Ergebnisse der Genetik der Muskeldystrophien.
Jong, J. G. Y. de: Dystrophia Myotonica, Paramyotonia and Myotonia Congenita.
Kloepfer, H. W.: Muscular Dystrophy of the Duchenne Type Transmitted as an Autosomal Recessive.
Walton, J. N.: The Inheritance of Muscular Dystrophy.
Welander, L.: Homozygous Appearance of Distal Myopathy.
Gamstorp, I.: Adynamia Episodica Hereditaria.
Bruins, J. W., and C. H. Simons: Hereditary Diplegia Spastica.
Huizinga, J.: Hereditary Acromelalgia.
Vogel, F.: Elektroencephalographische Untersuchungen an normalen Zwillingen.

E: CYTOLOGY—RADIATION GENETICS

Chairman: P. C. Koller.
Brandt, A. E.: Integrated Exposure from Radioactive Fallout as Basis for Estimating Genetic Effects.
Sonnenblick, B. P.: Radiation Genetics and the Individual.
Sachs, L., M. Danon, and D. M. Serr: The Prenatal Diagnosis of Sex in Humans.
Danon, M., and L. Sachs: The Sex Chromosomes in Human Intersexes.
Riis, P., F. Fuchs, S. G. Johnsen, J. Mosbech, and C. E. Pilgaard: Cytological Sex Determination in Disorders of Sexual Development.
Fuchs, F., E. Freiesleben, E. E. Knudsen, and P. Riis: Antenatal Detection of Hereditary Diseases.
Schultz-Larsen, J.: Some Observations on the Submicroscopic Structure of Mammalian Chromosomes.
Leuchtenberger, C., D. R. Weir, F. Schrader, and R. Leuchtenberger: Decreased Amounts of Desoxyribose Nucleic Acid (DNA) in Male Germ Cells as a Possible Cause of Human Male Infertility.
Doepfmer, R.: Uber die Morphologie und die Motilität von Hodenspermatozoen.
Schultz-Larsen, J., and R. Hammen: Electronmicroscopic Examinations of Sectioned Human Sperm Cells.

THURSDAY, AUGUST 2

PLENARY SESSION

Comparative and Experimental Pathology in Relation in Human Genetics.
Chairman: W. A. Mijsberg.
Nachtsheim, H.: Vergleichende und experimentelle Erbpathologie in ihren Beziehungen zur Humangenetik.
Giordano, A.: Hereditary Diseases of the Osteo-cartilagineous System. Comparative Morphological Basis.
Leuchtenberger, C.: Studies on the Cytoplasmic Inclusions Containing Desoxyribose Nucleic Acid (DNA) in Human Rectal Polypoid Tumors Including the Familial Hereditary Type.
Motulsky, A., R. Huestis and R. Anderson: Hereditary Spherocytosis in Mouse and Man.
Gentics and Medicine.
Chairman: R. M. Stecher.
Reed, S.: Counseling in Medical Genetics.
Gedda, L.: L'importance de la génétique humaine pour la médecine.

SECTIONAL MEETINGS

A: INTERNAL MEDICINE

Biochemical Genetics.
Chairman: Stanley M. Gartler.
Adlersberg, D.: Genetic Aspects of Atherosclerosis.
Edgar, G. W. F.: Inborn Errors of Lipid Metabolism.
Pfändler, U.: La manifestation hétérozygote et homozygote de certains troubles du métabolism (Porphyrie, Cystinose, Lipidoses).
Lamy, M., and J. Frézal: Gargoylism.
Milch, R. A., and H. Milch: Direct Inheritance of Human Alcaptonuria.
Anderson, E. P., H. M. Kalckar, K. J. Isselbacher, and B. Waage-Jensen: A Specific Enzymatic Defect in Congenital Galactosemia. (Read by S. M. Gartler.)
Hsia, D. Yi-Yung: Detection of the Heterozygote in Phenylketonuria.
Lehmann, H., and B. Ryan: Familial Low Pseudocholinesterase Levels.
Rheumatology.
Chairman: U. Pfändler.
Stecher, R. M.: Heredity of Several Types of Arthritis.
Blécourt-Meindersma, T. de: Hereditary Factors in Some Rheumatic Diseases. (Read by A. Polman.)
Thymann, G.: Polyarthritis in Twins.

B: PSYCHIATRY—MENTAL DEFICIENCY—PSYCHOLOGY

Chairman: F. J. Kallmann and Ö. Ödegaard.
Lewis, A.: The Offspring of Parents Both Mentally Ill.
Slater, E.: An Investigation into the Children of Consanguineous Parents.
Ödegaard, Ö., and H. Herlofsen: A Clinical Study of Psychotic Patients of Consanguineous Parentage.
Essen-Møller, E.: A Non-selective Study of Families in a Rural Population.
Mitsuda, H.: Klinisch-erbbiologische Untersuchung der endogenen Psychosen.
Koch, G.: Ergebnisse aus der Nachuntersuchung der Berliner Zwillingsserie nach 20-25 Jahren.
Philipsen-Prahm, H.: On Mental Illness in Twins.

C: POPULATION GENETICS — ANTHROPOLOGY —BLOOD GROUPS

C1: Anthropology—Physical Growth.
Chairmen: Karl Saller and W. W. Howells.
Gates, R. R.: Records of Y-inherited Hairy Ears in India.
Jungeblut, C. W., H. O. Goodmann, F. J. Kallmann, and B. Roth: Preliminary Twin Data on the Salivary Excretion of a Receptor-destroying Enzyme.
Owen, J. J. T., and G. A. Harrison: The Application of Spectrophotometry to the Study of Skin Colour Inheritance.
Harrison, G. A., and J. J. T. Owens: Family Studies of Skin Colour Inheritance.
Holt, S. B.: Quantitative Genetics of Dermal Ridge-Patterns on Fingers.

Pons, J.: Genetical Intercorrelations betweeen Several Dermatoglyphic Traits.
Sekla, B.: Heredity of Radical Loops and Arches in Finger Prints.
Lindegård, B.: Body Build and Physical Activity.
Otto, W.: Biorhytmische Beobachtungen an verschiedenen Wuchsformen.
Sontag, L. W., and S. M. Garn: Human Heredity Studies of the Fels Research Institute (Studies on Growth and Development).
Tanner, J. M.: Prediction of Adult Body Measurement from Measurements taken each Year from Birth to Five Years.
Das, S. R.: A Family Study of the Heredity of the PTC Taste Character, Applying Fisher's Method of Analysis.
C2: Blood Groups and Immunogenetics.
Chairmen: J. A. Fraser Robert and L. J. Unger.
Levine, P.: Blood Groups and Immunogenetics. Rare Red Cell Genotypes—Some Illustrative Cases.
Boorman, K. E.: Mother-Baby Blood Group Distributions.
Cotterman, C. W.: Somatic Mosaicism for Antigen A_2.
Grubb, R.: Hereditary Serological Human Serum Group.
Watkins, W. M., and W. T. J. Morgan: The A and H Character of the Blood Group Substances Secreted by Persons Belonging to Groups A_1 and A_2.
Woolf. B.: ABO Incompatibility in Haemolytic Disease of the Newborn.
Bentall, H. H.: Blood Groups and Disease.
Buckwalter, J. A.: Disease Association of the ABO Blood Group.
Clarke, C. A., and P. M. Sheppard: The ABO Blood Groups in Duodenal Ulcer.
McConnell, R. B., and P. M. Sheppard: The Secretor Character and Disease.
Mosbech, J., and M. Hauge: ABO Blood Groups and the Secretor Character in Patients with Achlorhydria.

D: NEUROLOGY

Chairman: L. v. Bogaert.
Stevenson, A. C.: The Familial Incidence of Congenital Malformations of the Central Nervous System Using as Propositi Autopsy Cases.
Lindau. A., and C. A. Larson: Capillary Angiomatosis of the Central Nervous System. 1) Denomination and Nosological Limitation.
2) Genetic Aspects.
Refsum, S.: Heredopathia Atactica Polyneuritiformis.
Sjögren, T.: A Clinical and Genetic Study on Tremor Essentialis in a North-Swedish Population.
Brage, D.: Genetical Studies of Two Families with Congenital Nystagmus.
Wiel, H. J. van der: Hereditary Congenital Facial Paralysis.
Novizki, I.: Heredofamilial Bilateral Facial Paralysis.
Novizki, I.: Approach to a Classification of Neurologic Genetic Diseases.

F: OPHTHALMOLOGY

Chairman: P. Waardenburg.
Franceschetti, A., and D. Klein: Two Families with Parents of Different Types of Red-Green Blindness.
Stillmunkes, J. Ruffie et R. Huron: Sur l'hérédité du daltonisme. Nouvelle observation d'une fratrie issue de deux daltoniens de types différents.
Kherumian, R. et J. Moullec: Dyschromatopsies et Gémellite monozygote.
François, J.: Heredity of Disc-shaped Cataract.
Nizetic, B. et D. Sakic: Dégénération nodulaire de la cornée (Groenow) liée a la couleur de l'iris. (Pedigrée d'une famille.)
Meyer-Schwickerath, G., and E. Grüterich: Microphthalmos Combined with Dyscrania and Dysphalangia.
Edmund, J.: Blefarophimosis Congenita.

FRIDAY, AUGUST 3

PLENARY SESSION

Methods in Human Genetics.
Chairman: L. Gedda.
Fisher, R. A.: Methods in Human Genetics.
Sutter, J. et L. Tabah: Méthodologie de l'isolat.
Waardenburg, P. J.: The Twin Study Method in Wider Perspective.
Verschuer, O. Frhr. v.: Uber den methodischen Beitrag der Zwillingsforschung für die Humangenetik.
Walker, Norma F.: Determination of the Zygosity of Twins.

SECTIONAL MEETINGS

A: INTERNAL MEDICINE

A1: Cancer—Dermatology.
Chairman: O.-Frhr. v. Verschuer.
Cancer.
Gianferrari, L.: Actualités en matière de oncogénétique humaine.
Koller, P. C.: The Role and Importance of Mutation, Variation and Adaptation in Malignant Growth.
Bernardi-Ronzoni, M. G., R. Paoletti et A. Serra: Recherches sur l'hérédité dans la maladie de Balzer et Ménétrier.
Cresseri, A., L. Gianferrari, P. Malcovati, G. Morganti, and P. E. Beolchini: Genetical Researchers on Uterine Cancer.
Morganti, G., L. Gianferrari, A. Cresseri, A. Arrigoni et G. Lovati: 1) Recherches clinico-statistiques et génétiques sur le néoplasie de la prostate. 2) Recherches clinico-statistiques et génétiques sur le néoplasie de la vessie.
Wolley, G. W.: Genetics and Leukemia.
Peller, S.: Genetics of Childhood Cancer.
Nielsen, A., and J. Clemmesen: Cancer among Twins in Denmark.
Dermatology.
Aschner, B. M., H. O. Curth, and P. Gross: Genetic Aspects of Psoriasis.
Schnyder, U. W., and D. Sommacal-Schopf: Erythro-Keratodermia Figurata Variabilis.
A2: Otology—Miscellaneous—Odontology.
Chairman: M. Lamy.
Otology.
Pfändler, U.: Une forme sublétale de la surdimutité récesstive.
Wildervanck, L. S.: Consanquinity and Congenital Deaf Mutism in the Netherlands. Are the Parents of Deaf Children Detectable as Heterozygotes?

Miscellaneous.

Codounis, A.: A Genetic Study on the Hereditary Methaemoglobinaemic Cyanosis.
Cuendet, J.-F. et E. B. Strieff: La relativité des notions mendéliennes classiques de dominance et de récessité.
Grebe, H.: Zwergwuchs als genetisches Problem.
Mijsberg, W. A.: Genetical-Statistical Data on the Presence of Secondary Oocytary Twins among Non-identical Twins.
Simonds, B.: The Collection of 300 Twin Index Cases for a Study of Tuberculosis in Twins and their Families.
Zonneveld, R. J. van, and A. Polman: Hereditary Factors in Longevity, a Statistical Evaluation.

Odontology.

Senyürek, M.: Time and Sequence of Genetic Changes in the Order of Eruption of the Permanent Teeth in White Stock.
Schulze, C.: Erbliche Strukturanomalien menschlicher Zähne.
Witkop, C. J.: Hereditary Defects in Enamel and Dentin.

B: PSYCHIATRY—MENTAL DEFICIENCY—PSYCHOLOGY

Chairman: E. Slater and E. Strömgren.
Maxwell, J.: The Relation between Psychology and Genetics.
Kallmann, F. J., and G. S. Baroff: Heredity and Variations in Human Behavior Patterns.
Jarvik, L. F., F. J. Kallmann, A. Falek, and M. M. Klaber: Intellectual Abilities in Senescent Twins.
Nyman, G. E.: The Physical Correlates of Personality.
Fuller, J. L.: Comparative Studies in Behavioral Genetics.
Spindler, P.: Eine Homologie des Verhaltens bei Mensch und Säugern. (Zur Vererbung von Verhaltensweisen.)
Deussen, J.: Erbbiologische Probleme bei der Jugendkriminalität. Kriminologische Untersuchungen an 500 jugendlichen Kriminellen.
Dencker, S. J.: A Twin Study in Mild Organic Mental Deterioration.
Silveira, A.: Twinning and Epileptoid Traits: A Research on 2060 Counselees of a Health Clinic.
Juel-Nielsen, N., and A. Mogensen: Monovular Twins Brought Up Apart. Preliminary Report of a Psychiatric-Psychological Study.

C: POPULATION GENETICS — ANTHROPOLOGY —BLOOD GROUPS

C1: Factors Influencing the Structure of Human Populations.

Chairman: L. L. Cavalli-Sforza.
Gini, C.: On the Physical Assimilation of Immigrants.
Gini, C.: On the Extinction of the Norwegian Colonies in Greenland.
Oostingh, R.: Religious Factors in Isolate Formation. (Read by A. Polman.)
Sutter, J. et L. Tabah: Structure démographique et génétique de l'isolat des Esquimaux polaires (Thule-Groenland).
Witkop, C. J.: A Study of Tri-Racial Isolates in Eastern United States.
Fracarro, M.: The Incidence of Cousin Marriages in Different Swedish Regions.
Sanghvi, L. D., D. S. Varde, and H. R. Master: Frequency of Consanguineous Marriages in Twelve Endogamous Groups in Bombay.
Serra, A.: Statistical Aspects of Consanguineous Marriages in Two Provinces in Northern Italy.
McKeown, T.: Sources of Variation in the Human Sex Ratio at Birth.
Takahashi, E.: On the Sex Ratio at Birth.
Vetukhiv, M.: The Longevity of Hybrids between Local Populations of Drosophila Pseudoobscura.

C2: The Genetic Structure of Different Human Populations.

Chairmen: A. E. Mourant and R. Ceppelini.
Moor-Jankowski, J. K., and H. J. Huser: Seroanthropological Investigations in the Walser and Romansh Isolates of the Swiss Alps and their Methodical Aspects.
Kalmus, H.: Gene Frequencies in Two Brazil-indian Tribes.
Kherumian, R. et J. Moullec: Répartitions des groupes sanguins A_1A_2BO, Rh, MN, et P en France.
Laughlin, W. S., M. P. Gray, and C. E. Hopkins: Blood Group Genetics of the Basques of Idaho.
Nijenhuis, L. E.: Blood Group Frequencies in French Basques.
Roberts, D. F.: Some Genetic Implications of Nilotic Demography.
Sachs, L., and M. Bat-Miriam: Finger Print Patterns in Jewish Populations in Israel.
Singer, R., and P. Brain: Haemotological Investigations and the Origin of the Malagasy of Madagascar.
Soliman, M. A.: Blood Group Gene Frequencies in the Egyptian Peoples and Their Racial Origins.
Twiesselmann, F.: Les métis bego-congolais.

F: OPHTHALMOLOGY

Chairman: A. Franceschetti.
Waardenburg, P. J.: Different Types of Hereditary Optic Atrophy.
Kjer, P.: Hereditary Infantile Optic Atrophy with Dominant Transmission. (Read by J. Edmund.)
Sorsby, A.: Some Dominantly Inherited Central Fundus Lesions.
Macklin, M. T.: A Study of Retinoblastoma in Ohio Families.
Alström, C.-H.: A Preliminary Report on an Investigation of a Monohybrid, Autosomal, Recessive Form of Congenital Retino-Choroidit.
Ohrt, V.: Ocular Albinism with Changes Typical of Carriers in a Danish Family.
Skydsgaard, H.: On Eugenic Problems in Prevention of Blindness.

SATURDAY, AUGUST 4

PLENARY SESSION

Biochemical Genetics.

Chairman: J. A. Böök.
Harris, H.: Biochemical Genetics.
Williams, R. J.: Biochemical Genetics and its Human Implications.
Lehmann, H.: Variations of the Haemoglobin Synthesis in Man.
Allison, A. C.: The Genetics of Sickle Cell Anaemia and Haemoglobin C.

Gartler, S. M., I. L. Firschien, and T. Gidaspow: Some Genetical and Anthropological Considerations of Urinary Amino Acid Excretion Rates.

SECTIONAL MEETINGS

C: POPULATION GENETICS — ANTHROPOLOGY —BLOOD GROUPS

Paternity Problems.
Chairmen: P. Moureau and E. Wolff.
Saller, K.: Anthropology and Paternity Problems.
Jonsson, B.: Blood Groups and Paternity Problems.
Bauermeister, W.: Biometrische Metoden der Vaterschaftsdiagnose.
Gürtler, H.: Genetic Evaluation of Blood-Grouping in 5,000 Paternity Cases with a Special View to Relative Probability of Paternity.
Harrasser, A.: Forensische Probleme der Anthropologischgenetischen Feststellung der Vaterschaft.
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Nijenhuis, L. E.: Probability of Paternity in Cases in which Exclusion by Blood Group Tests is not possible.
Wichmann, D.: Die Verwendung der Fussohlenbemusterung im Rahmen der Vaterschaftsbegutachtung.

G: GENETICAL AND STATISTICAL ANALYSIS

Chairman: J. A. H. Waterhouse.
Aubenque, M. J.: A. propos des statistiques générales de population.
Berglin, C.-C.: Generalized Formulae of the Penetrance Calculus.
Huizinga, J., Heiden, J. A. v. d.: Determination of the Mode of Inheritance from Estimates of Percentages of Concordance in Twins.
Keiter, F.: The Range of Applicability of Multifactorial Genetics to Man.
Oakland, G. B.: Linkage Studies with Multiple Allelomorphic Genes.
Smith, C. A. B.: Counting Methods in Genetical Statistics.
Linkage in Man.
Hösli, P., A. Hässig, and A. Franceschetti: Detection of Linkage between the Genes for the Blood Group System MNSs and the Gene for Ptosis Congenitalis Hereditaria Simplex.
Huron, R. et J. Ruffie: Sur l'hérédité de l'aplasie cutanée circonscite du vertex (avec indications des groupes sanguins).

MONDAY, AUGUST 6

PLENARY SESSION

Social Applications of Human Genetics.
Chairman: R. Turpin.
Larsson, T.: The Interaction of Population Changes and Heredity.
McKeown, T.: The Influence of Increased Expectancy of Life on the Genetic Identity of Human Populations.
Gordon, C.: The Place of Human Genetics in the Teaching of Public Health and Social Medicine.
Scheinfeld, A.: The Public and Human Genetics.

PANEL DISCUSSION

Epidemiological Control of Hereditary Diseases.
Chairman: G. Langfeldt.
Participants:
E. Essen-Møller, T. Kemp, J. Mohr, I. Novizki, O. Odegaard, A. Polman, M. Siniscalco, A. C. Stevenson, and J. Sutter.

THE GENE

"One of the most satisfying words in the English language is 'gene'. It has a singing resonance; it is refreshingly short for a newcomer; it is aptly derived, from the Greek 'to be born'. And it signifies an exciting fragment of life—the minute, ultimate unit of heredity.

"Genes occur by the thousand in every cell of every worm, bird, flower, human. Bacteria and wombats have them, orchids and spinach. And at this instant all the genes in all the living cells in the world are working full blast. At the least they are manufacturing copies of themselves for the next generation of cells. At the right time of the organism's life and in the right part of its anatomy, the genes concerned are organizing the materials which lead to the appropriate size, shape, color and behavior of that particular bit of animal or plant. Because of genes your finger prints and your blood group stay always the same. In color, genes control whether children have brown eyes or blue and insure that eyes stay brown and do not suddenly turn blue at, say the age of 53. Genes govern the color of a Hottentot's skin, of a butterfly's wings, of a sweet pea's petals, of the spotting of a Guinea hen or a giraffe. Environment and accidents may alter the effects of all these quite a bit, it is true, but the genes always hold the guiding reins. And, finally, the genes of the germ plasm preserve the entire inheritance of the next generation.

"In fact all genetics is a pleasantly forward-looking science. It has practically no history itself and, by necessity, is chiefly concerned with coming generations. Like atomic physics, it is ridiculously young for a full-fledged discipline. Its roots are firmly planted in both biology and chemistry, but scarcely any old-fashioned biology or chemistry is discernible within it. Modern zoology and botany got good starts in the fifteen hundreds, not counting Aristotle's foundation. The idea of evolution had recognizable beginnings long before Darwin. Astronomy has been plugging along for milleniums, with even the Chaldees and the Egyptians doing nicely respectable work. Practical physics was burgeoning from the days of the pyramid-builders and before. And all sorts of people, from the first makers of bronze to the alchemists had a hand in chemistry.

"But genetics, except for a few sensible observations on like-father-like-son, and the practical advantages of breeding cattle and corn from desirable stock, simply did not exist. And nobody had the foggiest notion as to how the good seed business worked, or why red hair or albinos or haemophiliacs cropped up only every now and then in a family of humans..

"You can give, to a year, the date on which the science of genetics was launched: it was 1864, and the story behind it is the story of a husky and cheerful monk named Gregor Johann Mendel.". . . .

(Jocelyn Crane; an Excerpt from Report No. 5 of the Fifty-fifth Expedition of the Department of Tropical Research, New York Zoological Society)

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Research on Intelligence and Personality (American Eugenics Society).
Fertility Control in the Light of Some Recent Catholic Statements, William J. Gibbons, S.J.
European Heredity Registers, Jan A. Mohr
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Dynamics of Human Populations, Dudley Kirk
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Future Fertility of American Women, P. K. Whelpton
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Effects of Differential Fertility on Genetic Endowment, Frederick Osborn
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Eugenics and Health

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Influence of Heredity on the Health of the Family, Barton Childs, M.D.
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A. Ghosh: The trend of the birth rate in India, 1911-50.

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